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THE PSYCHOLOGICAL REVIEW

FACTORS IN SPACE LOCALIZATION DURING INVERTED VISION. II. AN EXPLANATION OF INTERFERENCE AND ADAPTATION

BY HARRY EWERT

University of Vermont

It has been shown (9) that in all forms of motor activity and in all forms of verbal or conceptual behavior investigated, inverted vision serves as a distractor or causes interference provided such behavior is carried out in or involves the visual field. Behavior not directly or conceptually guided by vision remains unaffected. Perception limited to the visual pattern, including such figural aspects as relative size, shape, and color of objects, also remains unaffected. In motor tasks improvement in localization or adaptation to visual distraction progresses at a rate and manner which may be said to be typical of motor learning. In verbal or conceptual activity improvement in localization also progresses in a manner typical of such behavior, namely, in spurts as a result of suddenly completed learning or so-called 'insight.' These facts present two major questions: (1) What causes the interference when behavior involves the visual field? (2) What is a reasonable explanation of the adaptive process here involved?

In previous papers (9, 11) I suggested the following answers to these questions. Lenticular inversion effects a spatial transposition of the total visual pattern in all directions with reference to body movement and the normal receptor and conceptual processes. This means that localization based upon visual guidance will now be incompatible with localization based upon the simultaneous guidance of other non-

inverted or normal sense departments, with normal body movements including head fixations and eye reflexes of vestibular origin, and with habitual overt and conceptual behavior. Hence, adaptation will involve a co-ordination of behavior guided by inverted vision with behavior guided by unaltered sense data, with gross bodily activity both inherited and acquired, and especially with eye movements of non-ocular origin. It is thus evident that the types of interference and habituation here involved are very similar to those in non-experimental situations where inter-sensory guidance is at variance on the basis of attention or 'instruction set.' Examples of this may be found in all forms of inter-sensory-motor distractions. However, lenticular inversion introduces certain conditions which are not present in the interference resulting from the ordinary inter-sensory-motor and habitual antagonism. Foremost of these conditions are the antagonism between normal head fixations and lenticularly induced eye movements, antagonism between eye movements of ocular and vestibular origin, and antagonism between body movements of kinæsthetic and vestibular origin and those of ocular origin. In this paper I shall attempt to analyze these types of antagonism, to suggest an explanation of the adaptive process resulting from practice and finally, to suggest a theory of space localization in keeping with the facts of distorted vision. In order to accomplish these ends, I shall refer first to several foreign investigations on prism and mirror vision which have thrown valuable light on the adaptive process. Next I shall refer to some investigations of Dodge (**6**, **7**) and his students (**19**, **20**) on the reciprocal activity of the ocular and vestibular mechanisms, which likewise have rendered important information on the physiological mechanism of orientation.

In an attempt to correct disturbances in space localization in homonym hemianopsic patients there has developed a practical therapy which makes use of reflecting mirrors to replenish the eyes with the necessary stimulation for normal ocular activity. The chief cause for disturbances in localization is apparently not the sensory effect resulting

from the gap in the visual field, but lack of the necessary retinal stimulation to induce eye-fixations in all directions of the visual field. Since a rehabilitation of active orientation in the blind areas has been structurally impossible, attempts have been made to reflect the visual field, which in hemianopsia falls upon the blind area, so that it again falls upon the normal visual area. Although this brings about a distortion and an overlapping of objects in the visual field, it nevertheless furnishes the intact portion of the retina the necessary stimulation for eye-fixations in all directions and thus enhances, for example, the eye-hand co-ordinations.

The first attempt to correct disturbances in orientation by use of prisms is recorded by Best (**2**). He prepared spectacles which by means of a prism reflected the visual field 10° toward the blind side. This causes objects, which would fall 5° into the blind area, to be focused upon the normal visual area. Although Best does not report a fusion of the prismatically projected field with the normal field, he does report an essential improvement in orientation as a result of wearing these spectacles by one of his patients.

A similar attempt is reported by Strebel (**18**) who placed a 7° prism which deflected the field 3.5°, before the eyes of an hemianoptic patient. Strebel distinguishes two possible effects of this apparatus. He believes that the *optical effect* of the deviated visual field is accompanied by a *motor-set reaction* and that they compensate each other in the re-harmonization of the conflicting sensory guidance which the glasses at first cause. This is in agreement with Ewert's (**9**) interpretation of re-orientation.

Braunschweig (**3**) used prisms which were identical with those of Strebel, whose findings he also verifies. Braunschweig explains the helpful effect which these prisms have upon orientation on the basis that the decreasing extensity of the blind area caused less fatigue.

Igersheimer (**13**) introduced mirror-spectacles which, by means of mirror-prisms, reflect the entire visual field falling upon the blind area of total homonym hemianopsia so as to focus it upon the normal retinal area. The reflected visual

field is projected in accordance with the laws of bi-retinal inversion so that it harmonizes and overlaps with the corresponding visual field of the other eye. Practically, however, there result two non-identical visual fields which must be co-ordinated with practice. The reharmonization is reported to consist of an altered transposition of retinal points in the prismatically stimulated eye in such a fashion that the two spatial fields become congruous. However, complete congruity has never been reported. Igersheimer reports great improvement in localization with practice for both normal and hemianopsic subjects wearing the mirror-spectacles over a period of time. This, of course, is in agreement with the results of other studies of disoriented vision (**16**, **17**, **9**, **22**, **4**, etc.).

Metzger (**15**) has proposed an interesting explanation of the re-orientation which takes place while wearing the prism and mirror spectacles. His theory is of utmost importance to an understanding of space localization. It differs from conventional theories chiefly in that it fully disregards subjective phenomena and stresses the direct association between the physiological-optical stimulus conditions and the objectively demonstrable fixational movements of the eyes. Re-orientation is explained as due to the innervating effect that automatic eye movements have upon the gross musculature of the body involved in localizing movements. Wilbrand (**21**) has pointed out that if one places a prism before a normal eye, reflexive fixational movements occur toward objects in the prismatically deviated field. In homonym hemianopsia these sidewise fixations have been reported absent when the stimulating objects fall in the blind area of the visual field if the disturbance lies in the primary optical ganglion (*tractus opticus*). Both sensory and motor disturbances of ocularly induced fixational eye movements are known to appear in homonym hemianopsia. In both cases horizontal movements are affected and the reaction in one direction checked. By means of a rotating drum, which evokes eye movements in the normal half of the eye, it may be determined whether the fixational movements follow stimuli from the periphery to

the macula or in the reverse order (from the macula to the periphery). If movements in a given direction are thus found to be absent, no extension of the visual field will serve to reinstate again the fixational movements in both directions. However, an effective system will be one which deflects the direction of ocular stimulation so that an eye movement, which in the normal half of the visual field proceeds only from the fixation point to the periphery, will again be innervated by ocular stimulation to proceed also in the other direction, namely, from the periphery to the macula. The Igersheimer mirror spectacles fulfill these practical requirements. Their function is demonstrable on the basis of their optic nystagmus effects. That is to say, the absence of optically induced nystagmus toward the blind side may be demonstrated by means of a rotating drum and necessary recording devices. However, when the mirror spectacles are placed before the patient's eyes, nystagmus movements are again found to proceed in both directions. Metzger (**15**) points out that the re-orientation, which the continuous use of the spectacles facilitates, is relatively unaffected by subjective perceptual phenomena. He admits some sort of perceptual transformation which, however, he regards as entirely dependent upon oculo-motor processes. According to Metzger the all-important condition for a restoration of spatial orientation in hemianopsia is the restoration of those stimuli which induce fixational movements in both directions. Metzger further points out that a perceptual reorganization undoubtedly occurs in the retina resulting in a new central point for its pseudofovea. This, of course, implies that there is no strictly nativistic interdependency between retinal areas and inter-sensory-motor perceptual space as such for the total organism. Instead, perceptual space (up, down, right, left, near, far, etc.) is largely dependent upon motor integrating processes, such as directional fixation reflexes, conceptual habits, etc.

The initial interference during inverted vision is undoubtedly due to the antagonism between the lenticularly induced eye movements and those of vestibular origin and the resulting tonic influence that this antagonism has upon the musculature

of the whole body, and furthermore, the antagonism between lenticularly induced judgments of direction, distance, depth, etc., and the normally induced judgments of such spatial phenomena arising in other sense departments. To demonstrate the effect of antagonistic eye movements more clearly, we may again refer to the literature. It should be pointed out that, during normal vision, the slow phase of vestibular origin and the quick phase of ocular origin oppose each other (6, 7, 20). However, during inverted vision the slow phase of vestibular nystagmus is in harmony with the quick phase of ocular nystagmus or more precisely with eye movements necessary for clear fixation (11). Dodge (6) reports a reflex slow phase in direct opposition to head rotation. The quick phase becomes negatively adapted after prolonged rotation. This and other pertinent facts support the thesis, previously advocated by Ewert (10, 11), that exploratory eye movements or the fixational reaction patterns are subject to modification through practice. Such a view explains, at least in part, how a reharmonization may again be established between the disrupted functional activity of the slow and the quick phase of nystagmus which lenticular inversion brings about. *That is to say, during prolonged inversion, the disrupted quick phase is modified in such a manner as to become adjusted to the slow phase after the manner of normal vision.* However, before we develop this point further, let us explore more carefully the basic cause of the interference.

Dodge and Fox (7) report that the velocity of the slow phase during the visual pursuit roughly corresponds to the velocity of the moving object. The underlying mechanism of the quick phase is believed to be optic in origin and initiated by an impulse to fixate for clear vision. Dodge (6) first reported rivalry and competition between the slow and quick phases of nystagmus. More recently Travis (19) has studied the "effects of ocular pursuit on perception of rotary oscillation of the body" under the following conditions: "1. When the vestibular reflex was opposed by ocular fixation of an object moving with the body. 2. When both the vestibular reflex and the ocular pursuit tended to move the

eyes in the same direction. 3. When the body was oscillated and vision was excluded" (p. 416). It should be pointed out that condition 1 is probably the most common in everday life activity. It occurs during walking and other motor co-ordinations where head and other body movements are involved. Under these conditions the slow and the quick phase of nystagmus are antagonistic. This is also true when the body is stationary and perceived objects are moving, because ocularly induced eye movements now evoke tonic neck and body reflexes (**1**, **8**, **11**). Condition 3, of course, excludes eye movements of retinal origin. Condition 2, on the other hand, is an artificial set-up which is probably infrequently experienced by the human subject. It is, however, this condition which is brought about by the use of inverting lenses. *Lenticular inversion brings the slow and the quick phase of nystagmus into harmony with each other.* This is accomplished through the fact that optic fixations, whose underlying mechanism is the quick phase of nystagmus, are reversed when initiated by lenticularly inverted moving stimuli. During normal vision the quick phase is antagonistic to the slow phase and hence also to body movements induced by the slow phase. During inverted vision, however, the quick phase operates in harmony with or is ocularly stimulated to move in the same direction as the slow phase of vestibular origin.[1]

Since this analysis is supported by the work of Travis, let us examine his findings more in detail. "The analysis of photographic records of eye movements showed outstanding differential characteristics in the three situations.

"(*a*) When ocular pursuit and the vestibular reflex are in harmony the average normal pursuit latency was decreased approximately 50 percent and the number of corrective eye movements was decreased by approximately 40 percent.

"(*b*) When ocular pursuit and the vestibular reflex were opposed the average normal pursuit latency was in-

[1] This probably accounts for the swimming illusion which is experienced especially in the early stages of prolonged inversion experiments.

creased approximately 55 percent and the number of corrective eye movements was increased by approximately 40 percent.

"(*c*) There was a tendency toward increased amplitude of eye movements when they were congruent, and toward a decreased amplitude when they were opposed. These differences in amplitude, however, were statistically negligible" (pp. 429–430).

If we wish to apply these findings to phenomena of inverted vision and to localization in general, the question arises: how can a condition which reduces pursuit latency and number of corrective movements cause interference in localization and general orientation? As previously stated, lenticular inversion is similar to Travis' second condition in that it harmonizes the slow and quick phases of nystagmus. But Travis found that when ocular pursuit and the vestibular reflex are in harmony, the average normal pursuit latency and the number of corrective eye movements decreased. This suggests facilitation instead of interference, which is contrary to the effect of inverted vision which is known to cause interference in localization. How can these apparent contradictions be reconciled? The answer to this question is that inverted vision does not cause interference under all (intrasensory) but only under well-defined (inter-sensory-motor) conditions. No interference was found when localization took place outside of the field of view (**9**). This is similar to Travis' third condition and agrees with his findings. Furthermore, interference was less marked when localization occurred in the visual field, and when the behavior consisted chiefly of the conceptual type (**9**). Under these conditions the frequency of eye movements was undoubtedly reduced and eye movements which were present did not come into conflict with simultaneous overt activity and hence reduced the interference. Again, certain phenomena were actually accentuated. An example of this is the judgment of movement. All experimenters agree that perceived movement is accelerated by inversion. It is possible that here we have an

example of the facilitating effect of the harmonious functioning of the two phases of nystagmus.

The critics of the eye movement theory of space localization are justified in maintaining that for the visual sense department (intra-sensory stimulation) if taken by itself, inverted vision does not produce antagonism. It is only when overt activity and conceptual experience intitiated by the other sense departments come into play that antagonism results. This fact accounts for interference when overt and conceptual activity occur. It must always be kept in mind, however, that eye movements present only a part of the total antagonism which inversion produces.

Let us point out finally that nature has provided man with a visual mechanism in which ocular pursuit and vestibular reflexes and ocular pursuit and bodily activity function antagonistically according to the well-known law of reciprocal innervation. Hence, if a disturbance be set up in the ocular-vestibular or the ocular-motor mechanism, localization will undoubtedly be disturbed. In view of the fact that this disturbance is chiefly of the inter-sensory-motor type, it readily yields to adaptation. However, Wundt (**23**) and Gibson (**12**) also report adaptation to metamorphopsia or intra-sensory distortions. This may be explained by oculomotor processes of a different order. It should be explained that a point-to-point correspondence between retinal points and points of a projected image is no longer tenable in the light of present-day knowledge. Bartels (**1**) says, "During the fixation of a point, which falls upon the macula, we never deal with absolute fixation. A snapping-into-place mechanism as well as retinal fixation point does not exist, only a fixation disc. During static fixation there appear from 3000 to 4000 small movements per minute (Dodge and Dohlmann). The small action currents probably summate (Dohlmann). The larger movements have numerous functions such as compensation for head movements, muscle movements, fatigability of the retina, pulse movements, etc. (Dodge and Dohlmann)" (p. 275). A similar view has led Clark (**5**) "to a theory of stereoscopic vision which is based funda-

mentally on a rapid visual oscillation not only of the eyes together but from one eye to the other. This causes a series of intermittent retinal impressions which are of basic importance in stereoscopic vision and depth perception. The most important part of this fluctuation is the change in clear vision from one eye to the other. The resulting effect is similar to monocular parallax, but the change is from one eye to the other and has the distinct advantage of being very rapid. . . . Panum's areas, instead of being regions that, when simultaneously stimulated, permit clear vision, become areas within which intermittent stimuli are seen single. 'Corresponding points' lose their significance" (p. 95). The intra-sensory adaptation reported by Wundt and Gibson may thus be explained as an adjustment between eye movements of ocular and vestibular and central origin resulting in a slight shift or reorganization of the pseudofovea. The adjustment is necessitated by the prismatically induced disruption between the ocular and the vestibular mechanisms, both having as their function the maintenance of bodily equilibrium. The extent of such adjustment or shift of the pseudofovea, however, undoubtedly has its limitations. Hence a *complete* reorganization of intrasensory perception is highly improbable.

An Operational Theory of Space Perception

The so-called theories based upon nativism, empiricism, and eye movements respectively can no longer individually cope with the multitudinous facts of spatial orientation. Their basic assumptions are mutually reciprocal rather than individually exclusive of each other. The early scholars, working in this field, were impressed by the constancy of the 'immediately given' sense experience. So they formulated a nativistic theory of perception. However, in the light of present-day knowledge the *relative* constancy of the 'immediately given,' as compared with a motor response, is merely an acknowledgment of the fact that the different perceptual functions vary in degree of constancy or stability. Their degree of constancy is probably in part dependent upon their degree of complexity or interrelationship and in part

upon the constancy of the stimulus conditions which call them forth. That is to say, when a localizing response is a product of inter-sensory-motor activity it may be said to be relatively unstable. If, on the other hand, the localizing response is primarily a product of intra-sensory-motor perceptual processes (those arising in other sense departments being ruled out by the specific nature of the stimulus-response conditions), it may be said to be relatively more stable. Examples of the relatively constant intra-sensory spatial characteristics are size, shape, color, etc., whereas such inter-sensory-motor characteristics as distance, depth, etc., may be cited as less stable phenomena. Some types of perceptual processes are less in need of undergoing plastic adjustment by virtue of the relative stability of the stimulus conditions that call them forth, *e.g.*, shape of a familiar object. On the other hand, the perception of distance, which is an inter-sensory-motor and conceptual process, is constantly subject to adaptation by virtue of the relatively unstable stimulus conditions involved. This view is in general agreement with the advocates of Gestalt Psychology concerning the constancy hypothesis (**14**). However, in regarding the constancy of perceptual organization as dependent upon the *relative* intrinsic operational characteristics of a given spatial judgment, this view also accounts for those perceptual processes which are generally described as less stable. From this standpoint, space localization becomes a product of the two related processes, namely, native and empirical, rather than a sum of unrelated processes. Thus it stands in agreement with the most generally accepted view of 'nurture' and 'nature' as related to mental capacity. The so-called eye movement theory has even less claim for individuality than the nativist and the empiricist theories. In fact, it is not a theory at all. Eye movements may be regarded as a special type of empirical data in space perception or, more precisely, as a type of behavior which furnishes an integrating mechanism for inter-sensory-motor processes.

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THE DICHOTOMY OF SCIENCE AND PHILOSOPHY

BY J. A. GENGERELLI

University of California at Los Angeles

I

1. It is one of the more commonplace items of contemporary knowledge that science is becoming more 'philosophical.' To some this means that it is becoming somehow vaguer and less satisfactory, and one hears constantly from such persons the complaint that it is becoming increasingly difficult to 'put one's finger' on the objects of scientific investigation. To others with greater intellectual sophistication, it means that scientists are becoming increasingly conscious and correspondingly dubious of those barely formulated notions which form the very base of the processes of thought. As is well known, this was not always the case. Thus in the eighteenth and nineteenth centuries the prevalent attitude was that science is an enterprise seeking to discover the 'nature of the universe' and the unalterable laws which were presumed to govern it. Workers in the various scientific disciplines concerned themselves with the accumulation of facts and still more facts, and the study of the relationships between them, in the confident conviction that some day when the last fact was collected and the last relationship observed the human race would be in possession of a sure knowledge of the real world and its functionings.

The matter went further than this, however. Scientists, particularly in the nineteenth century, developed an ill-concealed superciliousness towards philosophy. So great, indeed, was the intellectual *naïveté* current among scientific men in the last century, so widespread the belief that the 'scientific' approach as it was then conceived would be sufficient to exhaust any problem concerning the universe, that in many quarters the words philosopher and philosophy became terms of opprobrium which were taken as synonymous with

hazy thinking and general muddle-headedness. Within the last thirty years this overweening confidence in what might be called the naturalistic view of knowledge and the universe has been seriously challenged in the exact sciences by the conceptual obstacles which have been uncovered in mathematical logic and theoretical physics. In the younger experimental disciplines, however, such as psychology, where there is a great deal of uncertainty and confusion as regards fundamental concepts, and consequently a corresponding amount of brashness serving as a compensating mechanism, the attitude is still only too painfully evident. Many a thoughtful consideration has been quashed there by the crushing epithet 'philosophical'!

The general problem of the strained relationship between scientists and philosophers is rendered even more piquant by the fact that philosophy in a large measure has given ample cause for disdain. Only too often systems of philosophy have been little more than well-knit and polysyllabic wish-fulfillments. A man gifted with eloquence and a fluent pen imposes his emotional and intellectual idiosyncracies upon a generation of readers, and among these there are some in whom he touches a responsive chord. If these have also eloquence and fluency in some measure, they act as secondary or resonating sources of influence and eventually the process results in an imposing ground swell. A new school of thought is born! Perhaps the school endures for some time and encrusts itself. In this event, there are sure to be rebellious souls with eloquence and fluent pens. By virtue of the same processes a reaction is formed. Thus are thoughtful and intelligent men subjected to this perennial teeter-totter of opinion which fills the pages of our text-books on the history of philosophy.

Further cause for the deprecation of philosophy is the fact that specialized scientific disciplines have arrogated to themselves provinces of knowledge which at one time or another—to use a phrase that has been hurled in the teeth of the beleaguered philosophers—were the object of 'philosophical speculation.' The history of science is no more than one long

story of these successive arrogations. The province of the mind was the last to be wrested from the unwilling hands of the parent discipline and made the object of scientific study under the dubious title of psychology. Here, too, progress is being made and along with this the confident claim of self-sufficiency.

What, then, is the place of philosophy and philosophic reflection in the intellectual life of our times? Is it to be looked upon as nothing more than the early history of man's intellectual efforts, much in the same way as alchemy is the historical antecedent of chemistry? Or can we look upon philosophic thought as an integral component in any effort which men may make to comprehend the universe? To these questions we shall address ourselves in the pages that follow.

2. Perhaps the surest way of depicting the 'philosophic temper' in its most generalized form and stripped of all its irrelevant excrescences is by the delineation and development of an actual problem which is typical. Let us envisage, therefore, a state of affairs in which scientists have at last solved the riddle of the universe and of life and when living organisms may be created synthetically in the chemistry laboratory. Fish, earthworms, mammals and *men* may be made to order and so well that they cannot be distinguished from those which are the resultants of the ordinary processes of reproduction and growth. These manufactured men might speak as we speak, cry over the same misfortunes, exult over the same victories, react as we do to the masterpieces of sculpture and music—all at the will and whim of the human creator responsible for their being. In this state of affairs, then, all the elements would be known, as well as *all* the laws of their behavior and combination, so that scientists could fabricate and distort *ad libitum*, or, rather, within the limits imposed by these self-same elements and laws. The laboratories, as places for discovery of new data, would be closed forever; science would become engineering, and universities, vast centers of technical instruction. Such would be the Golden Age of scientific omniscience and omnipotence—a level of attainment too frightful or too fantastic to be con-

templated soberly by our twentieth century mentalities, yet one which future millennia may well see realized.

Now in this state of exalted perspicacity there would be at least one unsolved problem—the problem of blue. We might very well have used some other color. In fact, any tone, odor, taste, touch or modality would be equally effective for our purposes here. From this it will be clear to those who are acquainted with the history of philosophical problems that we are approaching the question of *quality* and *awareness;* but for the sake of pedagogic simplicity we shall not introduce these time-honored concepts with their manifold implications. We shall speak simply of blue.

The colossal erudition of the high priests of this Golden Age of Science would most certainly enable them to trace step by step the electrical and chemical processes leading to, and indispensable for, the experiencing of blue. Their exhaustive knowledge of the subject of perception would permit them, in point of fact, to arouse in the experiencing subject an experience of blue of any desired saturation or shade in the absence of the impingement of any energy form upon the retinae. Conversely, in the absence of any knowledge on the part of the scientists of the nature of the energy form impinging upon the subject's retinae, they would be able to inform him of the nature of the perceptions which were occupying him at the moment. From this it will be seen that such an understanding of the subject of perception leaves nothing to be desired from a scientific point of view. Yet if one of these scientists, on the verge of his intellectual retirement, should ask himself the question, What is blue and what is its place in our scheme of knowledge, he would soon be nonplussed. His first impulse probably would impel him to look upon blue as the *reaction* of a physico-chemical system to electromagnetic waves having certain assignable characteristics. Before long, however, he would be forced to recognize in this 'explanation' a verbal tautology. For, obviously, his insight into the matter has not been increased by referring to blue as the 'reaction' of a physico-chemical system. In the same sense, we might say that rust is the reaction of iron to the

immediate presence of moisture and oxygen. This is all to the point if we wish to know how to produce rust; but it aids very little in getting an insight into the structural relation existing between iron, water, rust and oxygen. Such a point of view as this would never have led us to that exalted station where we can look upon the staggering multiplicity of phenomena in the world around us and see them as combinations of some ninety-three odd elements.

It is clear, then, that the particular organization of ions and colloidal particles which are present in the nervous system when the subject reports 'seeing blue' is not to be identified with the blue seen. On the other hand, to call blue a reaction of the nervous system to light waves is to miss the point, even though knowledge of the functional interaction between the organism and the circumambient environment is assumed to be such as to enable the scientist to control and predict to the minutest detail the intimate experiences of the subject. Hence further 'laboratory research' upon the matter under discussion would be out of the question. There would be no new observations to make; no new relations to embody in differential equations. In other words, the problem created by the non-identity of blue with electronic organizations is a problem which transcends the routine canons of experimental science.

At this point it is fitting that we should pause for breath and look carefully about us to appraise the situation, lest we find ourselves in a position which is entirely fatuous and posing questions which are irresponsible or trivial. What, then, is the nature of the 'solution' which we seek with respect to the above problem? In what sense is it a riddle; in what does the conceptual obstacle consist?

Perhaps the clearest approach to this question is to imagine a state of knowledge which is as much as possible the opposite of that invoked for our Scientific Utopia. This is not very easy, since none of us can reach adulthood without having a modicum of the cultural heritage of the western world ineradicably implanted in our neurones. But we can approximate such a state by imagining a society where there is a

minimum recognition of similarities and relationships between phenomena, and a minimum number of *concepts*. In such a society, no one would see anything common to the color of strawberries and that of oak-leaves in the fall; no similarity between water and milk; between oak trees and palm trees; between a cow and a panther; between the movement of a bird in flight and that of tree branches in the wind; between the impact of a stone from a sling and that of a fist. In this society, men's view of the world around them might be called anarchistic, inasmuch as the world which they experience consists of a welter of unrelated forms and qualities. For such men, the world would have a maximum of complexity, since there would be a minimum of repetition. Each object of experience would be a thing *sui generis* and therefore unique, without relation to anything else.

The most primitive societies known to anthropology are in better conceptual state than this. For in these we find manifested some progress in the classification of the multiplicities of sensory experience. When we consider the pre-Socratic Greeks, it is evident that we are confronted with a *Weltanschauung* which has gone far on the road of classification and categorization. The assertion that the universe is made of the four 'elements'—earth, air, fire and water—is indeed a far cry from the intellectual process of abstraction which recognizes that palm trees and pine trees belong to one class of objects while cows and panthers belong to another. But the animus and cast of mind which made both feats possible is essentially the same. When, therefore, a culture such as our own recognizes in the manifold of sensory experience two classes of phenomena, material and mental, this represents another milestone in the race-old journey of classification, another stage in the absorbing business of the conceptual simplification of human experience.

The problem of electrons and blue is consequently a special instance of this perennial attempt on the part of man to reduce the universe of his experience to terms of increasing generality and simplicity. More specifically, the conceptual obstacle here consists in the difficult task of subsuming blues and

electrons under a category which is sufficiently generalized and abstract to embrace them both. Perhaps this will be achieved through a repudiation of the two mutually exclusive categories of mind and matter as being in the nature of a misleading classification. This has, in fact, been attempted in a limited way by Hume, Ernst Mach and William James. But whatever the nature of the higher 'synthesis' which will turn out to be acceptable finally, it must be evident that this problem is merely one of the many which arise during the course of man's efforts at intellectual economy. It has been singled out for treatment here merely as a characteristic example of the intellectual vice which drives philosophers to look in 'dark cellars for black cats that aren't there.'

3. It is a fact not sufficiently recognized that the type of intellectual problem such as we have described, wherein the solution is to be found in the introduction of new concepts representing a 'higher synthesis' and hence a more generalized system, is one not confined to the remote outposts of intellectual exploration. The animus to higher and higher synthesis and consequent increasing generalization is a commonplace in the story of orthodox scientific thought. The history of mathematics literally bristles with such higher syntheses. Perhaps the most dramatic and spectacular instance of this in recent years is the development of the generalized geometries which reduce that once proud structure of Euclid to the status of a special case.

But, fortunately for us, it is quite unnecessary to venture into the forbidding realms created by Riemann, Minkowski, *et al.*, for illustration of this all-important intellectual idiosyncracy. The romantic story of conic sections is quite adequate to our purposes. To the casual eye, it is certain that there is nothing common to parabolas, ellipses and hyperbolas, except possibly the kind of graph paper on which they are drawn. It is, in fact, sufficiently unobvious that the ellipse and the circle are specific instances of a general class. But when it occurred to mathematicians to define a class of curves as the *locus of a point such that its distance from a fixed point is in a constant ratio to its distance from a fixed line*, the

human mind achieved a higher synthesis of incalculable significance and implication. For as this *constant ratio* contained in the definition assumes values which are less than, equal to, or greater than, 1, the curve is an ellipse, a parabola or a hyperbola.[1]

The experimental sciences also abound in instances wherein formidable obstacles are overcome by the construction of concepts of greater generality. It is, in fact, not too much to say that the most brilliant and productive periods in the history of scientific effort have followed immediately upon the heels of these purely conceptual reformulations. This fact stands in sharp contrast to the belief common to so many scholars and laymen alike that the scientist is some sort of glorified mechanic who tinkers and 'experiments' until something happens. Even scientists themselves very frequently look upon their calling as that of 'fact grubbing,' the business of patiently and stodgily accumulating facts and miscellaneous relationships between them, in the vague hope that some day something will come of it.

Perhaps the first dramatic conceptual reformulation of a large portion of the known universe occurred in the hands of Newton. To do this, he had to have his facts, true enough, and reliable ones; it is for this reason that we find him confessing that if his vision went farther, it was because he 'stood on the shoulders of giants.' These were the shoulders of Copernicus, Kepler and Galileo. But it is to his imperishable glory to have *conceived* those astonishing three laws which even today must leave any one but those who accept their dynamics in the most parrot-like fashion, literally gasping with amazement at the sheer audacity of them. For to say, for instance, that a body in motion will continue indefinitely in motion in a straight line unless deflected by some external force does more than set the problem on its head, although this requires courage and imagination enough; it brazenly denies a palpable observation as old as man. No experiment could accomplish this, no mere patient accumulation of facts;

[1] Neeley and Tracy, Differential and integral calculus, New York: The Macmillan Company, 1934, p. 64.

it was realized only by one of those sheer conceptual *tours-de-force* which those scientists who have never progressed mentally beyond the stage of setting up electric circuits and taking pointer readings are pleased to call 'philosophical speculations.'

The second great 'higher synthesis' in the inorganic sciences had to wait practically two hundred years for its realization. It was embodied in the principle of energy and its conservation. By dint of sheer repetition this concept which, with all that it implies, seems to verge so closely upon the mystical, has come to be accepted in an almost perfunctory manner both by those who use it as a basis for their investigations and by laymen. Yet consider the conceptual situation at the time of its promulgation by the abstract-minded Helmholtz. Here were sound phenomena, light phenomena, heat phenomena, the many phenomena presented by chemical reactions—each blissfully unrelated to any of the others. But they are miraculously subsumed under a new thought-form, if we wish to use that nebulous term, the concept of energy and its conservation. And what is energy? It is the *capacity* to do work! It is not even an existential, such as blue! Surely those who look upon science as conceptual beer-and-skittles and conceive it as purged of all 'metaphysical ideas' achieve this state of exalted smugness at the cost of insight into this extraordinary cultural process. For in the concept of energy we have a philosophical creation if there ever was one, a purely formal construction cut out of whole cloth designed deliberately to embrace classes of phenomena which seem to be worlds apart. The conceptual difficulties which lead to the formulation of the energy concept are, from a strictly formal point of view, no different from those resulting when the universe is boiled down to organizations of electric charges on the one side, and blues, pains and joys on the other.

The third great revolution in scientific *ideas* was precipitated, as everyone knows, by the relativity systems of Einstein. Already a vast amount of writing, both strictly technical and quasi-popular, has been inspired by the formal

creations of this spinner of conceptual systems; and nothing more need be said on this subject. But the sheer conceptuality of his structures is so patent that they have aroused even the dullest fact-monger. Perhaps the work of no other single thinker manifests so clearly and indubitably the philosophic business of seeking higher and higher syntheses. His prodigious reformulations of time, space, gravity, mass, energy are responsible for the state of acute 'concept consciousness' which exists among scientists at the present time. Here again we see a creation which is purely formal in nature and not falling out of the facts. The accumulation of facts, and the results of the Michaelson-Morley experiment in particular, did not suggest it; rather, the facts merely made it necessary for a philosophically-minded thinker to take the situation in hand.

4. From the foregoing, we must conclude that two of the most important preoccupations of the human mind are (1) the discovery and ascertainment of facts and the relations between them, and (2) the organization and arrangement of facts and their relations into what may be called conceptual structures having maximum simplicity from the point of view of the number of separate categories involved. These preoccupations should not be considered as distinct and unrelated activities. It is indeed doubtful whether, strictly speaking, they may be considered as processes which may occur independently of each other. While this question of the relation between facts and theory and all that it implies is an important one which arises unexpectedly in a variety of connections, we cannot consider it within the framework of the present discussion. We must therefore be contented with the bare assertion that an overwhelming portion of what is known as 'pure science' may be subsumed under these two categories. It would seem, however, that the majority of contemporary scientists are not aware of the fact that when we philosophize, the intellectual processes and procedures are not a whit different from those which constitute the so-called scientific approach. It should be evident, nevertheless, from the foregoing discussion that this must be the case. A philosopher

is a man who because of predisposition or training or both is predominantly concerned with the second of these two preoccupations. To say jeeringly that a person's treatment of a given problem is philosophical rather than scientific bespeaks, therefore, a lack of insight and knowledge of the scientific process itself and implies a distinction which does not exist. A denunciation of this sort, when analyzed, must imply either a challenge of the facts on the part of the accuser or an unwillingness to accept the postulates and classifications used to organize the facts, or both. But this is not peculiar to disagreements between philosophers and scientists. It is the basis for disagreements between scientists, just as it is the basis of disagreements between philosophers.

More specifically, the disagreement between scientists and philosophers is confined almost exclusively to the second of the two intellectual preoccupations which have been mentioned. Thus there are very few, if any, philosophers living who would be inclined to challenge the proposition that the adrenals secrete epinephrin, that the second cranial nerve is involved in vision or that the boiling point of ammonia is $-33.3°$ Centigrade. It is equally certain, however, that there will be many philosophers who will challenge the proposition that the universe may be considered as constellations of electric charges, or that it is possible to know the 'true nature of reality.' Such challenges clearly cannot be looked upon as the defense mechanisms of a priestly cult which has an intellectual vested interest. They are the inevitable reactions of any thoughtful mind which has considered the problems in question.

II

1. Not only is it impossible to single out the philosophic cast of mind for criticism without at the same time casting umbrage upon the most august achievements of science; there are reasons to believe that unsolved philosophic problems of long standing are in the nature of obstacles to the successful prosecution of that favorite enterprise of the modern spirit: the collection of facts.

The critics of philosophy and the philosophic temper are wont to charge that the problems of philosophy are so far removed from the province of human affairs and have such little relation to the world in which men live that they are little more than the idle puzzles of misdirected minds. Whether we accept philosophic parallelism or epiphenomenalism, they say, does not seem to matter in the construction of better radio tubes or the development of a new serum. In a word, the solution of philosophical problems does not butter scientific parsnips. Now if ever there was an instance of a sweeping generalization from insufficient evidence, we have it expressed here in the position of proponents of a science untrammeled by philosophic quibbling. Such a position fails to realize that science as we know it is at most not more than three thousand years old, and that it is very unlikely that the present year of grace offers us the last word in the *modus operandi* for investigating the universe.

In the exposition of the problem of blue under the conditions of scientific omniscience which were described, a tacit assumption was made which probably was not lost upon the careful reader. The assumption is that there could be a state of 'perfect scientific knowledge' without an antecedent solution of the purely formal problem of the existential status of consciousness in the universe. In other words, we have presumed that a system of science which is perfectly satisfactory from a purely operational or laboratory point of view is realizable in complete disregard of the mind-body problem and such other speculative problems which have been created by the intellectual perversity of philosophers. Now such an assumption may ultimately be shown to have been justified: but today we have at our disposal not a shred of indisputable evidence that this is the case.

2. It is most instructive to note that this possibility of the dependence of scientific progress upon pure philosophic groundwork which must seem so very remote and academic to the great majority of scientific workers, is an immediate and pressing problem in that branch of study which was the last to proclaim its independence of philosophy. In psy-

chology, the mind-body problem reaches out and casts its shadow over the very findings of the laboratory; it plays havoc with definitions, methodology and statement of problems. This is, in fact, the chief reason why psychology is cursed with so many 'schools.' We have those, for instance, who believe that an adequate and self-consistent science may be constructed in the total absence of any consideration of the fact of awareness or consciousness, and who look upon psychology as a biological discipline in the sense that it should invoke only biochemical considerations in the devising of its concepts and problems. These are well known as objectivists, or more loosely, as behaviorists. We have others who contend quite justly that such things as pains, joys, sorrows, blues, should be an integral part of the subject-matter of psychology; for if there is anything about man of which we are confident, it is the fact that he is conscious. These may be called 'subjectivists' for the sake of contrast. But this latter group of thinkers falls into several sub-groups who take varying positions in regard to the relationship between mental states—the blues, the pains, the joys—and the biochemical processes in the brain. There are three chief groups which may be mentioned: (1) the parallelists, (2) the two-way interactionists and (3) the epiphenomenalists. Those who are acquainted with the history of philosophic thought will recognize at once in these three groups, three classical positions which may be taken with respect to the mind-body problem.

Group 1 is exemplified by the 'configurationists,' who, while not making explicit philosophical commitments, seem to believe that conscious states parallel closely in organization and behavior the biochemical field processes in the cerebrum. In Group 2 might be included the earlier psycho-analytic system of Freud and the 'hormic psychology' of William McDougall. These two men and such others as constitute their disciples seem to agree, then, to the proposition that not only may biochemical processes influence mental states, but that these latter may, in their own right, institute biochemical processes. The majority of American psychologists are to be found in Group 3. These are of the belief that biochemical

processes are responsible for conscious experiences, but that such laws as seem to govern conscious experiences are referable to the equations which govern the biochemistry of the brain. It is implicit in this point of view that what we call a man's mind may be controlled to the minutest detail by modifying the nerve currents of the brain. This group of thinkers maintains, therefore, as an article of faith what the scientists in our Scientific Utopia were presumed to have realized as an actuality.

Now it must not be thought, as is so often done by psychologists themselves, that this state of almost ridiculous diversity as regards such apparently simple questions is merely an indication of fuzzy-mindedness. Anyone wishing to sustain such a position in formal debate will quickly discover to his discomfiture that this is not the case. In other words, on the basis of such information as is available at the present time, we may choose indifferently any of the enumerated positions with almost equal justification. Indeed, it is not at all evident that the continued accumulation of careful observations will change this situation. The problem is a purely conceptual one and not one of evidence. It is of a class with that of the existential status of blue in a universe of electrons.

3. It is evident how such a state of affairs must affect psychology as an experimental discipline. Where there is a lack of agreement upon fundamental premises, there must perforce be disagreement as to what constitutes a significant and critical experiment. For what one investigator considers an adequate 'explanation' another may consider as itself requiring 'explanation.' This may be illustrated by the following typical example. An investigator determines the amount of time required and the number of errors made by blindfolded subjects in learning to run an elevated finger maze. Now, after a group of subjects has learned such a maze under the above conditions, the maze is rotated through 180° in such wise that what originally was the starting point is now the endpoint, and *vice versa*. The subjects are informed of this state of affairs and are urged to learn the new path. It then becomes immediately apparent that some of the

subjects have very much more difficulty in the new situations than others. In fact, there are a few who remain distressingly confused for some time.

When this phase of the investigation is finished, the subjects may be examined for vividness of visual imagery. When this has been done, it is shown that those who have very vivid imagery have, in general, the least difficulty in mastering the maze after its rotation. There are some, in fact, who can trace through without errors after the first one or two trials. On the other hand, the relearning records of those who have dull or fragmentary visual imagery show a corresponding inability to reorient themselves to meet the demands of the new situation and hence are guilty of a greater number of stereotyped movements leading to errors.[2]

Now a little consideration of the experimental situation assures us of the 'justness' of these results. For clearly those who have vivid visual imagery are more likely to form a picture of the maze in their 'mind's eye' as a result of the extensive blindfold practice. When the maze is finally rotated, all they need to do is to rotate their image of it to the required degree and proceed in accordance with their new mental picture. Those who have no ability to form mental pictures, however, are faced with what is an almost totally new situation, and hence require more time and make more errors.

This will strike anyone who is inclined to accept the possibility of the influence of mental states upon nerve activity as a very satisfactory and sensible 'explanation.' But to those who are thoroughgoing epiphenomenalists and belong to Group 3, the above is not an explanation at all. For them, a complete understanding of the matter is not achieved until the actual biochemical processes which they conceive to underlie the various degrees of relearning capacity are minutely and fully described. They will therefore find it impossible to envisage or explain a psychological process of the above type in terms of images. The significant thing in this situation, however, is that a rigorous epiphenominalist, because of

[2] Davis, F. C., *Publications of the University of California at Los Angeles*, Vol. 1, 47–64.

his cavalier attitude toward subjective mental states, *would never have performed the experiment at all.* Being in such a 'frame of mind,' it might not have occurred to him, in the first place; and if it did, he would reject such an approach as 'unscientific.' This is an actual fact to which any psychologist over the age of thirty, having a reasonably wide acquaintance with the members of his profession, can bear witness. Thus in the hey-day of Watsonianism, anyone daring to speak of 'imagery' did so at the risk of incurring a pitying look from his colleagues.

Nor is the above an isolated instance where the wholehearted assumption of some 'philosophical' position influences our experimental investigations and our facts. The number of such instances in psychology is literally legion. We must content ourselves, however, with but one more illustration.

Perhaps the most striking example is the phenomenon of hysteria. A man at the front in war time becomes blind, although the most rigorous opthalmoscopic and neurological examination reveals no physical basis for the disfunction. Here is a physical incapacity which seems to have no physical basis. Now the only thing a thoroughgoing objectivist or epiphenomenalist may do at such a juncture is to believe that a physical basis exists, but that in the present state of our biological knowledge it is undetectable. He can, therefore, only wait, and perhaps add his bit to the increasing knowledge of neurophysiology and biochemistry in the hope that one day this mystery, too, will yield when all the 'facts' are known. Fortunately for our knowledge, not all psychologists are strictly 'objective' and 'scientific' in the sense in which these two terms are so frequently misused. For there are those who believe that fears, worries, anxieties—in short, mental states—are capable of bringing about physical incapacities. These beliefs are amply corroborated by fact, since the use of hypnotism, persuasion, suggestion either singly or in conjunction succeeds where all else has failed.

Again it is to be noted that even in the event a successful cure is achieved in this manner, the results are certain to be derided by those who consider themselves as 'scientific'

psychologists. These will point out that any attempt at an understanding of the phenomenon in terms other than those of biochemistry is, *ipso facto*, futile and illusory, since the 'true causes' underlying both the disease and the cure must lie, in the last analysis, within the nervous system itself.

4. There are many psychologists who are led by considerations of this kind to the intellectual whimsy that the business of science and of psychology in particular is to 'stick to facts' and avoid embroilments in 'philosophic speculations.' Such a point of view betrays marked disorientation with respect to the actual state of affairs. For, contrary to popular psychological opinion, it is not the pressure of brute facts which is responsible for the assertion that biochemical and physiological processes are the only valid and objective data which psychologists may accumulate and study with profit. Nor is it brute facts which are responsible for the cavalier attitude which behaviorists have taken toward consciousness, and for their favorite dictum that psychology should concern itself only with 'objective' facts and relations. Such a position is pure philosophy. When a psychiatrist informs us, therefore, that the 'true causes' of hysteria are to be found in the biochemistry and neuro-histology of the nervous system, he is speaking as a philosopher, whether he knows it or not. And, as we have seen, if all psychiatrists were to take unwittingly this particular philosophic point of view, a great many important facts would not be ours. If, on the other hand, the psychiatrist should tell us that he knows nothing of, and is not interested in, 'true causes' but is desirous merely of establishing, if possible, correlations between what are called biochemical processes and what are termed mental states, without attempting to decide which is the cause and which the effect, he is again speaking as a philosopher, whether or no he is conscious of the fact. For he is expressing thereby a position which has implicit in it definite philosophical assumptions and consequences. In effect, he is being an unconscious exponent of a philosophy approaching that of David Hume.

It is clear, then, that any scientific approach to psychological problems rests upon a body of implicit preconceptions. Further, it is self-evident that as we approach the matter with one set of assumptions or another, our findings will vary. Indeed, we have seen that under the guidance of certain preconceptions, there are facts which do not appear at all. Clearly, it is of the utmost importance to practical scientific progress in psychology that this fact be formally acknowledged; for as long as this is not done, we run the risk of neglecting many aspects of our subject. In other words, psychologists must face resolutely these various formal problems and come to some self-conscious decision in regard to them. The only way to insure a solid base for our scientific structure, it would seem, is by being at the beginning and throughout philosophers of the self-conscious and not the unwitting variety.

From the examples which have been given, it is plain that unless we strive to construct our conceptual forms and patterns as we go along, not even our precious facts are forthcoming. It is, indeed, not too much to say that one of the important factors responsible for the chaotic state of contemporary psychology is this intellectual stereotype on the part of psychologists which impels them to shy away from their most fundamental problems because they lead to 'philosophy.' This state of mind results in a feverish scramble for means, medians and correlations, a fetish for laboratory paraphernalia, a plethora of experimental papers—and a wealth of data whose outstanding characteristic is mutual contradictoriness.

In psychology, then, we have a field of scientific endeavor where significant and harmonious development is dependent upon conceptual formulations which are usually considered as being philosophical in nature. However, the fact that psychology as a province of specialized knowledge should be forced to face these fundamental questions at the very outset of its career may turn out eventually to be a piece of good fortune. For it may thereby show the way to those other

disciplines which had to wait until their adulthood for conceptual self-consciousness.

There is no reason to believe, furthermore, that an explicit formulation of the problem of blue or, more generally, the place of consciousness in the universe, is the only one which stands in the way of future advances in the experimental sciences. It is conceivable that the only universe which we may know is one which cannot be understood except as we proceed in accordance with a definite progression: in such an event it may be necessary to solve 'first' problems first and to give formal answers to such questions as "What is the nature of the knowledge given us by experience?" "What acceptable meaning can we give to such statements as 'The nature of reality'?" It is a curious fact not devoid of a certain element of irony that those who were openly scornful of the time-honored problems of philosophers have been forced to them by those very methods which were considered as rendering the process of 'philosophical speculation' an historical oddity. Thus the problems of Locke, Berkeley, Hume and Kant have been rediscovered by Bohr and Heisenberg.

III

It is hoped that the foregoing considerations have sufficiently demonstrated not only the intellectual fruitfulness of philosophy but its inevitableness. To express disbelief in philosophy is equivalent to expressing disbelief in Chinamen or polar bears. To be a philosopher is not to belong to a caste or a cult; it is to reflect on one or any of the most generalized problems which suggest themselves to the human mind. We are philosophers willy-nilly, whether we believe it or not. No scientist, however circumspect, can penetrate very deeply into any subject without involving himself in a number of assumptions which must be made tacitly unless they are recognized as such. But to recognize them as such implies the conception of other possibilities, and the business of conceptually envisaging the universe in various ways has since the dawn of known history been the preoccupation of those thoughtful men whom we have called philosophers.

Perhaps the mischievous dichotomy which consists in seeing philosophy and science as two mutually exclusive provinces in the realm of intellectual effort may be traced to the signal success enjoyed by specialization in the last two centuries. So overwhelming, indeed, has become the prestige of specialization that for many contemporary thinkers it has assumed the proportions of a fetish. As a consequence, they have submitted to the fatal whimsy of looking upon the world of knowledge as being patterned after the manner of University Catalogues of Courses. Nevertheless, it must be evident even upon relatively superficial inspection that there can be no place where science stops and philosophy begins. The distinction is not only impossible but augurs confusion both in the direction of conceptual analysis and that of 'fact-finding.' There have been only too many philosophers, it is true, who have tried to save their souls and defend their pieties by means of syllogisms, without first inquiring into the validity of either their souls, their pieties, or their syllogisms. But, on the other hand, is not the history of science strewn with as many pious intentions and blasted conceptual hopes? Was it not Newton who hoped to reveal by his scientific investigations the nature of God's handiwork? And, with the exception of Ernst Mach, what nineteenth century scientist of the first water was not convinced that scientific investigation revealed the 'true nature of reality'?

Indeed, it must be apparent to anyone who will consider carefully the history of philosophical efforts that the gravest sin of philosophy does not consist in its having been so far removed from the immediacies of everyday life, but in not being abstract enough. For the apparent endlessness and pointlessness of many philosophic quarrels arise immediately from the fact that the human mind has not satisfactorily answered the most generalized and abstract of all the riddles, namely: What can it mean to offer a satisfactory solution to any problem; what can it *possibly* mean to bring a difficulty within the realm of understanding? Once again we find ourselves confronted with the curious possibility which we

considered in the foregoing pages: that the abstractest problems must sometimes be solved *first* before further progress can be made. It is for this reason that possibly the most fundamental intellectual work of the century is being done in the symbolic and mathematical logics; for these are indeed the most philosophical of all subjects.

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PSYCHOLOGY TOMORROW [1]

BY CHRISTIAN A. RUCKMICK

University of Iowa

The field of psychology tomorrow is inviting but it is as boundless as all out-of-doors; the writer may have shown some competency as a systematic and experimental psychologist but not as a prophet; and the outlook, if you take a chance at it, may be as bleak and black as that of Ethiopia—stifling hope, stemming enthusiasm, and smothering inspiration. Why forecast? Why not get to work and let the future take care of itself? In a recent speech which Mr. James P. Warburg delivered before the Chicago Association of Commerce he is reported to have said, "Ordinarily, in order to do justice to a subject which lies more than 90 per cent in the future, one would have to be a clairvoyant, or an astrologer, or at the very least a New Deal statistician." [2] While uninformed people expect much of a psychologist, any one of these three rôles could not possibly be in his repertoire.

As an officer of our thriving organization might be moved to give an annual account of the state of that body, so it might conceivably be incumbent upon him also to have a 'weather eye' toward the future. Feeling the present pulse of experimental and theoretical achievement and knowing something about the case-history of the science, it might not be too arrogant or too foolhardy to look ahead, if for no better reason than to indicate its possible career and to lay plans for its further steady growth. If psychologists are qualified to give advice in the matter of adjusting individuals to do their future share of the world's work, to assist in shaping educational programs, to visualize the growth of educational and other institutions, why should not the same operation of

[1] Address of the president before the Midwestern Psychological Association, Evanston, Illinois, April 25, 1936.

[2] J. P. Warburg, What of 1936, *Vital Speeches*, 2, 1936, 267.

creative imagination be applied to the science of psychology itself? If planned societies and planned economic structures are possible, why should there not be planned sciences? Pieces of experimental work are planned, theses are planned, whole areas of research are planned: why not a planned psychology?

Let us not misunderstand one another. Admittedly mistakes in such a planning will be made. Prognostication usually proceeds with a comfortable margin of error—comfortable for the sake of refuge later on! But these errors of forecasting have historically hardly ever discouraged further planning. Pillsbury and others have clearly shown that at best inductive reasoning is partly deductive and because of the frailties of the human mind always subject to some extent to the vagaries of trial and error.[3] In other words omniscience is no perquisite to the power of prediction. Omniscience would not need prediction. And, after all, we could still safely try negative eugenics. Experience should have told us, or be still telling us, what to avoid and what to inhibit. All students of the history of psychology should be guarded against the pitfalls which have appeared in the path of progress. Psychology thus becomes wise from its own experience. Even under the present circumstances certain obstacles and encumbrances still beset us; in the future others will appear. But the point is that psychology has passed through the awkwardness of its adolescence and now has attained its adulthood. At the dedication of the University of Iowa laboratories more than five years ago one of the distinguished psychologists present stated that we no longer had to emphasize the fact that psychology is a science. Within the year a *factual* textbook on psychology furnished a comprehensive answer to those sceptics who not many years ago doubted whether psychology had accumulated a real body of fundamental facts.[4]

[3] W. B. Pillsbury, The psychology of reasoning, New York: D. Appleton, 1910, pp. 188 and 242–254; J. Dewey, How we think, Boston: D. C. Heath, 1910, pp. 79–96, 150–156; E. Rignano, The psychology of reasoning, New York: Harcourt, Brace, 1923, pp. 115–120.

[4] E. B. Boring, H. S. Langfeld and H. P. Weld, Psychology, a factual textbook, New York: John Wiley, 1935.

Long ago Ebbinghaus said that "Science must prefer the admission of honest poverty to the appearance of wealth" and he regretted the fact that the problems of psychology, outside those of philosophy, are of the most disputed character. In his preface he remarked ". . . and with every forward step which it has taken in its knowledge of facts, the squabbles concerning the proper interpretation and significance of these facts have scarcely diminished." [5] That might be said, perhaps, of any science, for a colleague in chemistry once stated that "a true scientist raises more problems than he solves." In the last few paragraphs of his 'History of Experimental Psychology,' Boring, who is also one of the authors of the 'factual' textbook above quoted, relieved himself as follows:

"There is a criticism of modern psychology, often spoken and sometimes written, that the new science has not quite succeeded, that it has been, as compared with its ambitions, relatively sterile, that it set out to study mind by the experimental method, and that it has gained a mass of knowledge about sensation (which the physiologists might have gained), a little else, and nothing of great moment about the rational mind, the personality, and human nature. This criticism, when expressed by philosophers, may be suspected of reflecting the disappointment of philosophers at the turn which the new psychology took, or even of their reaction to the negativism of psychologists toward philosophy. However, it is not at all impossible that psychologists themselves might express some dissatisfaction with the advance of psychology, were they relieved from the necessity of defending themselves." Thereupon he confessed "to a certain disappointment of his own that experimental psychology has not accomplished more than it has in its seventy years of life." He gave at least two reasons for its shortcomings: (1) "psychology has never had a great man to itself," and (2) "psychology has never succeeded in taking philosophy to itself nor in leaving it alone." In response to the first reason, he shed a ray of hope, because "there are signs that psychologists are ready for a great man

[5] H. Ebbinghaus, Grundzüge der Psychologie, Leipzig: Veit & Co., 1902, vii.

or a great event, for they seize eagerly upon every new movement that aspires to greatness." Thus far, however, the great leaders, of the magnitude of a Helmholtz or a Darwin, have either been shared by the other sciences or they have been like Wundt massive in their systematic production. The latter-day saints have led forth "self-conscious schools" and have therefore enjoyed only a brief place in the sun. Psychological journals are "weighted down with 'theoretical' papers that are really the expositions of psychologists, untrained to philosophy but writing on philosophical matters. Incompetent work can be ignored, but a division of the mind within psychology is not healthy." [6]

Discounted in part on the basis of weariness from the completion of a scholarly and comprehensive historical survey, these remarks still stand as the honest convictions of a competent appraiser. Especially significant are they in view of the high merit which Boring in his own presidential address, delivered before the American Psychological Association less than a year earlier, assigned to the personality of the scientist who could wage controversies because he should be capable of assuming the rôle of "the prosecutor or the judge at will." [7] The full force of the accusation, then, can hardly be assuaged by a wave of the hand as one might ward off the remarks of Driesch who about six years before wrote an entire book concerning the critical situation in psychology. For him there are "four problems 'of the very first order' in modern psychology that have reached a critical point nowadays." They are: "the *mind-body* problem, the problem of the *unconscious*, the problem connected with *psychical research*, and strange to say, perhaps, the problem of the fundamental materials and laws of *normal psychology* pure and simple." [8]

Thus from the foregoing passages we can easily gather the hint that 'all's not well.' If we pass from such explicit statements to actual deeds, we need only remind ourselves of the many overt acts by way of reform which certain groups

[6] E. G. Boring, A history of experimental psychology, New York: Century Co., 1929, pp. 658–661.

[7] E. G. Boring, The psychology of controversy, PSYCHOL. REV., 36, 1929, 97–121.

[8] H. Driesch, The crisis in psychology, Princeton: Princeton University Press, 1925.

of protestant psychologists are committing. We have passed through a whole succession of schools: structural, functional, 'act,' existential, purposive, dynamic, behavioristic, 'Gestalt,' configurational, topological, and still other forms of psychology have come and gone. They have seen the light of day, but the Caesarian dictum, *veni, vidi, vici* was not fulfilled: they have never conquered. Such an indictment perhaps can not fairly be directed against some of the current systems, because only time will tell whether they too will pass away. Insofar as they have modified ever so slightly our concept of what is meant by mind or mental process, or of what we wish to include in the domain of psychology, or by what procedure we wish to approach our subject matter and gain the facts of psychology, they have been beneficial. Certainly the fundamental concepts of any science periodically require revision and recasting: history shows that this process has been going on continually and is likely to go on indefinitely.[9]

Back of this process of revision lies the inherent capacity of the human mind to deal with more and more refined and plausible concepts. The new vector method of mental analysis would not have been possible in the time of Weber and Fechner; John Locke could not have entertained the notions of configurations and of fields of forces. He had enough to do to struggle with a complexity of ideas. Now even the layman has a rough, though it be very rough, idea of relativity. These matrices of meaning which provide the current vehicle of thought and the medium of exchange of thought will continue to come and go. Someone could write a fascinating popular article on the cast-off clothing of knowledge, Socrates's *daimon*, Aristotle's 'breath of life,' Galen's 'bodily humors,' or 'pneumata,' the 'phlogiston' and 'caloric' of the physico-chemical sciences, Driesch's 'entelechy,' and dozens of other concepts which have done their duty for the day and then quietly have gone to their reward. Who knows whether the germs and genes of our day have come to stay forever? The icons of the past become the ions of the present

[9] R. Müller-Freienfels, The evolution of modern psychology (trans. W. Béran Wolfe), New Haven: Yale University Press, 1935, pp. 3–22.

and so the endless procession casts successive shadows on the walls of Plato's cave as the scientists of the world intently gaze and sometimes mistake the shadows for the reality.

Our quarrel, then, is not with these ever-changing symbols of thought and meaning, our quarrel is rather with those protestant psychologists who, because they have discovered a more convenient concept, attempt to revamp the whole face of psychology. This face-lifting technique then appears to imply that because the nose has been remodelled the whole character of psychology has been uplifted. No such development has ever been recorded in history. There never has been any radical change in the way in which psychology has applied itself to its task. There has been a shifting of emphasis in regard to the various aspects of experience which were attacked. Numerous techniques have been added, others have been cast aside. New psychologies have been proclaimed from time to time, but the old stayed with the new and the new became old. In fact, we might be tempted to say that the new *was* old before it became new. Behavioristic approaches were in constant use in child psychology and in animal psychology before Watson sounded the call to battle on all fronts. Both Wundt and Titchener used them under the name of the expressive method. Gestalt goes back at least to John Stuart Mill almost a century ago in his reference to the chemical analogy as a substitute for his father's mechanical composition of the human mind, to Ernst Mach in 1885 when he proposed his sensations of spatial forms like geometrical figures and sensations of temporal patterns as in the case of melodies,[10] to Bentley who in 1902 reviewed the systematic positions of von Ehrenfels, Meinong, Schumann, and Cornelius and suggested in place of 'funded contents' the concept of incorporation.[11] What is new, often, is the vigorous protest that the other points of view are fruitless and erroneous. Just this protest is the serious mistake, but then

[10] See E. Heidbreder, Seven psychologies, New York: Century Co., 1933, pp. 335–337.

[11] I. M. Bentley, The psychology of mental arrangement, *Amer. J. Psychol.*, 13, 1902, 269–293. His earlier paper, The synthetic experiment, *ibid.*, 11, 1900, 1–21, set forth an instance of experimental analysis followed by experimental synthesis.

the cry must be loud and long or else it will not be heard. The upshot usually is that, as the smoke of battle subsides, hostilities seem to give way to fraternization. To vary the figure of speech the new pupils have made their little recitation, have acknowledged the applause and are settling back now to work with the rest of the class.

In keeping with the forward look with which we began, may we then expect in the next decade or two fewer and fewer *new* psychologies. We have had the 'Psychologies of 1925' and the 'Psychologies of 1930.' The 'Psychologies of 1935' did not appear. Is it not a fairly safe wager that there will be no more 'Psychologies of 1940, 1945,' *etc.*? May we not confidently hope that such youthful outbursts will be restricted to theories and techniques that apply not to the whole of psychology, but to more limited and specific problems within the entire realm of psychological enterprise? Hypotheses and theories, along with other assumptions, are as necessary to science as are the experimental and statistical data—both tentative theories before empirical investigations are made and well formulated theories after the facts have been ascertained. Then follows some form of integrated systematization when a large number of items are collated and elaborated into comprehensive treatises. Boring probably did not call that philosophizing. But the presumption that a new school is to be set up, followers solicited, and a new turn given to the whole of psychology, is scientifically as undignified as it is impossible of accomplishment. Already there are many signs on the horizon which typify not centrifugal but centripedal tendencies [12] in psychology. Psychology is a coöperative enterprise. Bickerings within the precincts of our far-flung family are both unseemly and unwise. We may make allowances for them among ourselves,

[12] This tendency was well presented in a series of invited papers which were read at the 25th Anniversary of the Inauguration of Graduate Studies at the University of Southern California, Los Angeles, Nov. 21–23, 1935. The conference of psychology under the chairmanship of Dean Carl E. Seashore focussed its attention on the 'Converging Trends in Psychology.' R. S. Woodworth admirably stressed this position in his 'Contemporary Schools of Psychology,' New York: Ronald Press, 1931, Chap. 7, entitled "The Middle of the Road."

but we are often called upon to explain to outsiders that psychologists are fairly friendly to one another at conventions and that generally we speak the same language with few, if any, explosives or execrations to punctuate our discourse. Let it be set down then, that the first forecast which we can make—a forecast and not merely a devout wish—is that fewer radically new 'schools' or systems of psychology of a protestant nature will appear in the future. Progress will come by gradual revision, through the use of more adequate concepts as the human mind becomes capable of grasping them, but a revolutionary and iconoclastic change is not to be expected either in this country or in our own science.

Secondly we may look confidently forward toward a closer affiliation between psychology and physiology. There is no reason for confusing the two disciplines as far as central problems are concerned. We who are gathered here are not primarily interested in problems of the body as *body*. But whatever our convictions or guess may be concerning the psychophysiological relationship, we are more and more realizing that mind and body are in close partnership and that the body is the closest neighbor to mind. All the stimuli which are transformed into human experience from the taste of tea to the fortissimo of a grand finale must be channelized through the body. Troland did us all an inestimable service when he labored greatly and produced his four-volume work on 'Psychophysiology.' [13] The number and quality of papers which are being published in which mental facts are correlated with bodily processes is really outstanding. Some years ago test procedures and statistical evaluations, important as they are and will continue to be, were encroaching on the safe balance of our national and sectional programs. A perusal of the papers offered before the 43d annual meeting of the American Psychological Association will reveal the fact that over half of them are directly or indirectly concerned with bodily processes. Cameron, Franz and Lashley have fundamentally revised, and perhaps have radically upset, our

[13] L. T. Troland, Principles of psychophysiology, New York: D. Van Nostrand Co., 1929–1932, Vol. I–III.

notions of localization of cerebral functions, but the work of spring house-cleaning, especially in the regions of the attic, is going merrily on and gradual order will no doubt soon replace chaos upstairs as well as downstairs in the human domicile.

Not only general problems of bodily and mental activity, or more minutely the functions of the central and autonomic nervous system and the networks, but also the more restricted correlations of physiological mechanisms with the known phenomena of mental operation are calling for increased attention. We are literally amazed at the tremendous increase of effort in the direction of solving the all-pervasive problems of learning which is exhibited today. For a lifetime interest in this field and for a competent and consummate skill in systematizing the field we have only to consider Carr who has quietly but efficiently sponsored the persistent investigation of learning. Looking to the future, we can confidently count on the careful and critical work of McGeoch who last year proposed that in addition to the cross-sectional view of mind, the long-sectional view of mind reveals two major vertical dimensions, that of maturation-degeneration and that of learning.[14] He also sanctioned the statement that "the capacity to learn has been proposed as the criterion for the existence of mind." [15] Recently some sixty-two pages of the *Psychological Bulletin* were devoted by him to a summary of the field. Obviously we may point out that we are not only on much firmer ground now than when Ebbinghaus and G. E. Müller gave us their pioneering start, but that the problem of retention, when mechanically and functionally conceived, is resolved into the problem of neurological forces as Köhler, Koffka and Lewin are so effectively telling us.[16]

[14] J. A. McGeoch, The vertical dimensions of mind, PSYCHOL. REV., 1936, 43, 107–129.

[15] J. A. McGeoch, Learning (Chap. 13), 'Psychology, a Factual Textbook,' 1935, pp. 300–343.

[16] K. Koffka, Principles of Gestalt psychology, New York: Harcourt, Brace, 1935, pp. 52–67; W. Köhler, Gestalt psychology, New York: Liveright, 1929, pp. 271–274; K. Lewin, Dynamic theory of personality, New York: McGraw-Hill, 1935, pp. 62–65.

When we realize the tremendous effect that the Coghill [17] theory of progressive individuation has had on physiological interpretations of the process of development, we are prepared for the rapid progress which genetic studies of child growth will make in the next few decades. This fresh intellectual concept has already modified our exposition of evolution according to the Darwinian formula of the survival of the fittest in terms of environmental forces and according to the doctrine of reflexology, which also placed the burden of responsibility on stimulation from without the organism. We already had some indication that the child life and the mental life of the lower animals were most profitably interpreted in terms of physiological processes. Numerous studies could be cited to validate that statement, but we might mention the comprehensive treatment of the former by Child [18] and the masterly presentation of the latter by Holt.[19] But Gesell and others have more recently shown how the maturation of neurological and muscular systems from within the organism play an important part in the learning process and in the gradual adjustment of the individual.[20] It need hardly be pointed out that much of the experimental work and certainly most of the systematic treatises on animal and child psychology are heavily loaded with the factor of bodily process.

On coming into closer quarters with this growing tendency to approach mind in its various forms from the physiological and biological angle, we find typical instances in the sensory fields, such as in the area of auditory sensation and in the emotional region. Since this matter is so important to the entire point of view of physiological psychology, we must pause for an interpolated note.

[17] J. E. Coghill, The neuro-embryologic study of behavior: principles, perspective and aim, *Science*, 1933, **78**, 131–138; Anatomy and behavior, Cambridge: University Press, 1929.

[18] C. M. Child, The physiological foundations of behavior, New York: Henry Holt & Co., 1924.

[19] E. B. Holt, Animal drive and the learning process, New York: Henry Holt & Co., 1931.

[20] A. Gesell, Maturation and infant behavior pattern, PSYCHOL. REV., **36**, 1929, 307–319.

The difficulty is closely analogous to the intrusion of the statistical treatment in psychology. The definiteness of quantitative measures can be a vast help or it can be a sad hindrance. The finality of figures is something to be watched. So is the concreteness of bodily reaction. Take for instance the startling example of the experiments of Wever and Bray, when tones were led into the external meatus of the ear of a decerebrate cat and then greatly amplified electrical currents were tapped from the eighth cranial or auditory nerve. These amplified currents were then conducted to a loud speaker in another room where human subjects could not only hear the conversational tones of the person speaking into the cat's ear but they could identify the voice of the speaker. This achievement was based upon the remarkable discovery of the now well-known Wever-Bray effect.[21]

Such instances and many more are all very picturesque. But what do they mean psychologically? Even if it is possible to obtain the sound from the auditory nerve after it has gone through the live mechanisms of the cochlea, it is not at all safe to conclude that under normal conditions the cochlea has not the function of selection that Helmholtz attributed to it. The electrodes were stuck crudely into the bundle of fibres which constitute the auditory nerve. It has been found that there is an intact segregation of single fibres from the various turns of the cochlea. In fact by the use of more refined electrical techniques, Culler, Finch, Girden and Brogden at Illinois [22] and Lurie, Davis, Stevens, and Derbyshire at Harvard have conclusively proved that the cochlea is tolerably selective in function [23] up to about 7000~.

In other words, physiological and biological techniques may be impressive, but, unless carefully interpreted, they are also sometimes misleading. When you have carefully

[21] E. G. Wever and C. W. Bray, The nature of acoustic response: the relation between sound frequency and frequency of impulses in the auditory nerve, *J. Exper. Psychol.*, 13, 1930, 373–387.

[22] E. A. Culler, An experimental study of tonal localization in the cochlea of the guinea pig, *Ann. Otol., Rhinol., and Laryogol.*, 44, 1935, 807–813.

[23] H. Davis, A. J. Derbyshire and M. Lurie, A modification of auditory theory, *Trans. Amer. Otol. Soc.*, 24, 1934, 110–119.

guarded your inferences on the physiological level you have still a psychological hurdle to jump. For example, since animals and infants can not give verbal reports of what they sense and feel, the experimenter is often driven to take as his mental cue some slight catch or 'flutter' in the breathing reflex as Horton and Wever have done or some front paw withdrawal technique as Culler and his colleagues have developed it. Wever and others admit that the respiratory flutter may be due to an 'emotional' excitement and Culler has found that the withdrawal reflex of the paws can be conditioned to a bell in the complete absence of the cerebral cortex. If the anticipatory pawing reflex occurs, then, as a cue to the 'hearing' of various tones, can we be sure that 'hearing' took place as a mental phenomenon in the conscious sense or was the reflex merely a subcortical affair? The question is raised merely to illustrate some of the difficulties which we encounter when we use apparently convincing physiological techniques and are not extremely attentive to our psychological bearings. The point is particularly important because the term 'sensation' may be legitimately used in either the physiological or the psychological sense.

The problem comes closer to our own experimental interests when we consider a situation which has been debated for nearly half a century beginning with the Jamesonian story of the hypothetical bear in the woods which in the now famous formula was supposed to evoke the reflexes that, when 'felt,' produced the emotion of fear. The critics pointed out that the bear had to be fearsome as a perceptual and therefore conscious experience in order to start reflex action. Bears in zoological gardens did not usually start such reflexes and therefore stimuli merely as stimulus patterns could not be held responsible for producing the reflex chain of events. The Gestalt psychologists would here have the floor and point out that the difference lay in the diverse nature of the entire field of forces. More recently Cannon waged an argument with Wheeler and his colleagues over very nearly the same point among others.[24] Wheeler quite rightly raised the issue

[24] W. B. Cannon, Again the James-Lange and the thalamic theories of emotion, PSYCHOL. REV., 38, 1931, 281–295.

whether a decorticated animal with a hypothalamus intact could have an emotion of rage.[25] Truly Cannon frequently referred to this sort of behavior as 'sham' rage, but then he did also refer to the hypothalamus as the 'seat' of the emotions in the lower animals and by inference in man (the so-called 'thalamic' or 'emergency' theory). Two curious factors should at once annoy the well-trained student of animal behavior. The hypothalamus is usually conceived as a ganglionic platform through which all sensory pathways go—except the olfactory nerve which has, of course, probably the most direct connection between sensory path and cerebral center of any sensory neural mechanism. A by-pass or secondary neural connection to the hypothalamus does exist, however. The alarming point is that in all of the animals used in the Cannon experiments the olfactory sense is extremely important, relative to man, in the mental life of those animals. What does the poor animal do, then, when its most important avenue of reception for fear producing stimuli does not lead directly to this 'seat' of the emotions? Secondly we are generally now committed to the fundamental principle, as Troland rightly points out,[26] that, when in the higher animals the cerebral centers are more intricately developed, we have a right to expect them to usurp some of the functions of the lower centers. At least at every stage of development of the central nervous system the total picture is changed functionally as well as structurally.

When finally Landis, Cantril and Hunt gave us their outstanding contribution concerning the difference between 'cold,' *quasi* and real emotions, as introspectively reported, the half-century old question was at least tentatively settled.[27] The stimulus-situation was identical: adrenalin chloride was intra-muscularly injected. The physiological or bodily response was identical: certain phenomena of sweating, shiver-

[25] E. B. Newman, F. T. Perkins, and R. H. Wheeler, Cannon's theory of emotion, a critique, PSYCHOL. REV., 37, 1930, 305–326.

[26] *Op. cit.*, Vol. III, 317–321.

[27] H. Cantril and W. A. Hunt, Emotional effects produced by the injection of adrenalin, *Amer. J. Psychol.*, 44, 1932, 300–307; C. Landis and W. A. Hunt, Adrenalin and emotion, PSYCHOL. REV., 39, 1932, 467–485.

ing, *etc.*, were produced. But the individual mental processes were remarkably different. One person had no emotion—the emotion was 'cold' and impersonal; another had a *pseudo* or *quasi* emotion, reminiscent of another occasion when he had experienced a similar set of symptoms, but not an emotion in the present; and a third individual really felt the complete emotion of fear. They therefore conclude that neither an account of the stimulating conditions nor a description of the bodily symptoms affords a complete or true picture of the emotion which is after all known only through experience. The cognitive report is essential. The names of the emotions as we have them, behaviorists notwithstanding, are derived from their experiential significance. It is essential to notice that many such conclusions are being arrived at by experimenters who through training and perhaps through inclination would find it easier and more satisfactory to avoid the introspective interpretation.

The reason for laboring this point so long lies in the very preponderance of physiological and biological work which the writer is advocating. Inferences from one sort of experimental area to another bear watching. Physiological approaches, especially with the lower animals as subjects, are fascinating, fruitful and frequent. They will become much more frequent. But in spite of their fascination they will become fruitful only if we make the logical leap from physiological techniques and terminology safely into psychological ground.

Analogous to the preceding argument and outlook is the growing tendency to rationalize the application of electrical techniques in connection with experimental problems. We have already briefly alluded to the matter. Again there is fascination. With the availability of numerous amplifying circuits based on the characteristics of various oscillating tubes or valves, the degree of possible intensification of current runs at present into the hundreds of thousands in actual experimental work. There is in fact almost no known limit of amplification which can be utilized when necessary without the introduction of serious extraneous errors. The

accompanying distortion is often a relative matter and, with the refinements of electrical filters and 'bucking' circuits, errors are being rapidly reduced. There are also difficulties with electrodes and other accessories, but again human ingenuity is becoming increasingly competent to cope with such physical matters. The next step is to make the relatively distortionless amplification so great that we can do away with electrodes and pick up a varying bodily or endosomatic current inductively in the surrounding field. Steps are already vaguely reported in that direction. Our own laboratories and doubtless many others now have completely shielded rooms—that is rooms entirely covered with metal or screen wire which is grounded—so that electrical recordings may be made with a minimum of outside interference. Under such conditions experiments never contemplated in the early laboratory days are being carried out. For example a technique has been used which picks up the chirp of a male katydid, carries the sound to a waiting female katydid on the other end of a telephone line so faithfully that she orientates her antennae and her body to the sound of her hero as it emerges from the receiver.[28] Many others could be cited. Hence the literature is teeming with studies on the electrodermal response, or the so-called psychogalvanic reflex, 'brain waves,' action currents, refractory phase, chronaxie and other aspects of neural functioning.

The basis for all this activity lies in the fact, of course, that live cells manifest an electrochemical disturbance. Besides, the explanatory concepts of the physical sciences are reaching out everywhere into the electrical field. As with many other concrete evidences of the mental life, we must first admit the tremendous possibilities which the future holds in store for us from that angle and then we must guard against the too hasty conclusions which we tend to draw from such easily seen and heard data. Already grievous mistakes have been made by many of us in our interpretation of the results. Nothing does quite so well as a good footing in psychology.

[28] E. G. Wever and C. W. Bray, A new method for the study of hearing in insects, *J. Cell. & Comp. Physiol.*, 4, 1933, 79–93.

Never should we lose sight of the fundamental fact of experience to which we must ultimately return as to our homeland after visiting foreign countries. While the boundaries of the various sciences are no longer rigorously drawn, the land is still there. It is much like many suburban residential sections where no iron fences or hedge-rows demarcate the areas which each individual calls 'home.' So the various sciences have taken down their artificial looking fences; trespassing may even be tolerated; but the identity of the territory is still intact. Much may be gained by utilizing the techniques and the points of view of neighboring sciences. Strength again comes from united effort and a united front. Psychology, however, should not sell out to other disciplines but do everything to maintain the integrity of its own point of view and the body of facts which have been accumulated.

There is another ray of hope which reaches toward the horizon of the future. The light is of tremendous power and the location of it is well within the realm of psychology. It draws its main source of energy from that territory. We refer to quantification, psychophysical measurement, and statistics. With an ancestry in the Weber-Fechner type of psychophysics, it came to manhood with the establishment of test-procedures and personnel work during the war, and now has borne fruit both in pure and applied psychology. In fact from several points of view it is on an equal footing with the experimental procedures of the laboratory and certainly it is a strong secondary defense against uncontrolled conditions. The writer has directed more than one thesis which used the utmost care possible in setting up isolable conditions in the laboratory and which then employed statistical procedures to check the data against other possible sources of error. It is well-known, for example, that we still have such accusations as 'laboratory atmosphere' and indoctrination to face in experimental work! Certainly we can look confidently forward in the direction of using mathematical concepts more broadly, even in conjunction with experimental work, and also in the direction of refining these concepts for our use. The organization of the psychometrists of the country has

already been accomplished but the critical work of Kelly, Spearman, Thurstone [29] and others needs no commendation. Just now we are also becoming aware of the persistent interest which a group of psychologists is showing in an outgrowth of higher mathematics closely allied to symbolic logic, namely topology. Lewin and others have marshalled not only the notions of the physical sciences but the fertile ideas of higher mathematics to do service in psychology. Vectors, matrices, multiple factors, positive manifolds, centroids and many other terms are taxing not only the memory but the understanding of the common 'garden variety' of psychologists. We thought that we had mastered the phi-gamma hypothesis and Urban's tables, but now we stay up late trying to make sense out of

> The orthogonal transformation by which a factorial matrix F may be rotated into an orthogonal positive manifold F_p contains $\frac{1}{2}r(r-1)$ independent parameters, where r is the number of columns of F.

We do not intend to criticize such developments negatively. We see in them on the other hand, the help that comes from careful quantitative analysis. The only caution which we can not afford to throw to the winds and which most of these statistical investigators have already publicly expressed is that figures must not speak louder than facts. No amount of quantification can convert basically unsound data into wholesome and healthy scientific food. Quantification can lay bare the errors that have insidiously crept into otherwise careful work; it can go so far as to create new analytical concepts of mind, especially in those greatly complicated functions of the personality, like social attitudes, motivations, drives, and individual traits; but we must not become obsessed either by the concreteness of statistical figures or by terminology *per se*. The development here, when properly safeguarded, will undoubtedly give us new worlds to conquer.

This leads us directly into the realm of application and service. Many of us feel that we should not only advocate

[29] L. L. Thurstone, The vectors of mind, Chicago: University of Chicago Press, 1935. K. Lewin, Topological psychology, New York: McGraw-Hill, 1936.

a dignified peace inside of the family of psychologists, but we should also make contributions of value to the world of education, art, industry, politics, and even to the inherent problems of the social structure, like marriage. Several conservative books on applied psychology have appeared and in his recent presidential address before the American Psychological Association Poffenberger has made a strong plea not only in favor of developing the techniques and the technical workers capable of giving scientific integrity to *homo psychologicus* but also in favor of guarding the public against incompetent and unscrupulous mendicants.[30] Often, of course, valuable products of the laboratory have been inspired by the public need and have then been returned with compound interest to the public for its own use. Gault, no doubt, was at first primarily interested in the vibratory sense as a scientific phenomenon. As a result of his continued investigations he has given us a typical illustration of the application of pure research to the needs of the deaf who can now enjoy symphonic music and other forms of radio entertainment through the tactual sense modality.[31] Many other instances could be given of the practical value of experimental and statistical research. We ourselves have supplied such information to the field of the musical and graphic arts, toward the improvement of reading and speech defects, toward the elimination of undesirable features of the motion pictures, and toward the proper evaluation of public opinion. Many other laboratories have found similar outlets in advertising, personality adjustments, selection of personnel, revision of the curriculum and methods of teaching, and in other directions too numerous to mention even in passing. At a sectional meeting last December in St. Louis, Hoisington also made a very strong plea for increased activity of this type.

Possibly we have been altogether too retiring, but let us hope not too diffident, in making our knowledge more easily

[30] A. T. Poffenberger, Psychology and life, PSYCHOL. REV., 43, 1936, 9–31.

[31] R. H. Gault, 'Hearing' through the sense organs of touch and vibration, *J. Franklin Inst.*, 204, 1927, 329–358; Pleasurable reactions to tactual stimuli, in *Feelings and Emotions: the Wittenberg Symposium*, 1929, 247–255.

available for general use. That is the inherent nature of scholars. We must not overlook the fact, however, that the repercussions from the vast, though relatively uncontrolled, laboratory of public needs, are exceedingly useful provided no undue claims for certainty and accuracy are made in the first place and provided we submit nothing for use in this manner before we have tested it as thoroughly as we can in the alembic of the laboratory. Thus many new refinements are brought into being, new questions are raised and answered, when pressure comes from application. The development of radio circuits is an illustration in point, but many others could be given. Great expansion may safely be predicted in this field within the next few decades.

And finally we may look not only wishfully but confidently forward toward greater coördination of research. In many instances the best efforts of experts in the several subdivisions of psychology are lost through poor articulation and integration. What can be done in the way of pooling intellectual resources on given problems has been clearly proved by the subsidies afforded through the Spelman Fund, through the Payne Fund, and through similar organizing agencies. Dunlap most clearly and succinctly summarized this proposal in the form of a central psychological institute or bureau of standards.[32] The writer has for years advocated an institute for research in auditory phenomena. In the meantime, with all its faults and dangers, we shall see the gradual emergence of more and more coördinated effort in the direction of solving problems of the first order. The idea is tolerably sound and the method is beyond the experimental stage.

Thus we are back at the beginning. Planning a science as a whole is the natural outgrowth of planning specific pieces of research: it is the legitimate extension of what occurs continually in our laboratories. Somehow the best thought of our best men has not been stimulated in this direction, but inherently a planned psychology when wisely contemplated for relatively brief periods at a time would be an interesting experiment. A sufficiently able committee,

[32] K. Dunlap, The outlook for psychology, *Science*, 69, 1929, 201–207.

representing various phases of the subject, could profitably indicate the major problems. Logically it would mean merely the extension of the same process that is going on in the various individual laboratories. It would not be regulatory or mandatory in character, but merely suggestive. If that proposal seems too radical to hope for within the next few decades, we might more easily follow the plan of increasingly coördinated research outlined for five-year periods with annual conferences and critical reviews of progress. That has already been tried with outstanding success in several instances and some failures in others. The failures have come from poor perspectives and inadequate psychological sponsorship. Organizations like the Psychological Corporation and our own national and sectional bodies should be consulted and the machinery placed in their hands or into the hands of a committee representing the various professional units. Several projects could be organized simultaneously. But it seems a pity that scientifically we are still operating by chance when larger perspectives and broader horizons are confronting us.

The several outstanding tendencies, which were outlined as characteristic of the psychology of today when extended to the psychology of tomorrow, would gain due balance and proportion when framed with forethought and purpose in a planned psychology. The elimination of competitive schools, the reference to physiological interpretation, the proper use of electrical techniques, the application of quantitative measurement and analysis and the coördination of independent researches all lead to the same goal: progress in the understanding of human experience.

[MS. received May 29, 1936]

DEMANDS AND CONFLICTS [1]

BY EDWARD CHACE TOLMAN

University of California

I would distinguish three problems in a psychology of motivation: (1) that of the nature and definitions of 'demands'; (2) that of the mechanisms by which secondary demands are derived from primary ones; and (3) that of the laws by which competing demands, whether primary or secondary, interact, *i.e.*, the laws of conflict.[2]

1. Definitions

Demands I conceive to be a type of 'intervening variable' (6). That is to say, they are one of the several types of entity which psychology has found it convenient to postulate as intervening between such independent variables as maintenance-schedules, heredity, previous training, age, special organic conditions, and environmental stimulus-objects, on the one hand, and the dependent variable of resulting behavior, on the other.

In terms of physiology such demands are presumably a matter of autonomic and endocrine activations. But, at present, we know but little of this, their underlying physiology. And, even if we did know more about their physiology, we, *as psychologists*, would still be interested in them primarily because of their external environmental relations. For it is these environmento-behavior relations which we, as psychologists rather than physiologists, take as our subject-matter.

[1] Read before the Western Psychological Association, Seattle, June 19, 1936, as part of a Symposium, entitled: 'In what Terms are Motives and Conflicts to be Described?'

[2] For the best and most illuminating recent presentations of the facts of motive and conflict see: Stone's 'Motivation: Drives and Incentives' (in Moss's *Comparative Psychology*) (4); Young's 'Motivation of Behavior' (8); and Lewin's 'A Dynamic Theory of Personality' (3).

By *primary demands* I conceive ones that are expressed (in the first instance) either in a *final consummatory* behavior or in a *final avoiding* behavior. In the former case, we may call them the primary *positive demands* and, in the latter case, the primary *negative demands*.

A completely satisfactory list of the final consummatory behaviors, with their contained *positive demands*, has not as yet been agreed upon for man or indeed for any of the higher mammals. I suggest, however, for illustrative purposes, the following: eating, drinking, copulating or otherwise stimulating the genitalia, suckling the young, urinating, defecating, resting, sleeping, stimulating sensitive zones other than the genitalia.

Further observation and experimentation will undoubtedly lead us to add to and subtract from this list. My point here is simply that, whatever the final list, each behavior in it may be said to contain and to define a demand for a certain internal bodily state, *i.e.*, a 'consummation' (the physiology of which latter, however, we in many cases do not know). What we can and do know is that when such a behavior has been performed enough times in immediate succession, it ceases and will not tend to be repeated again until after a time-interval —during which latter new values in some characteristic controlling 'maintenance-schedule' will have had time to develop.

A primary positive demand is thus an intervening variable which intervenes between some controlling maintenance-schedule, such as a food schedule, a drink schedule, a copulation schedule, an oestrus cycle schedule, an exercise schedule, or the like, and the readiness-to-go-off of a corresponding final consummatory behavior. It must be noted further, however, that this final readiness-to-go-off of such a consummatory behavior is determined also by two other features, (i) another intervening variable to be called a set of 'hypotheses,' and (ii) the presentation of an actual consummatory stimulus-object.

But these relationships can be made clearer by a geometrical scheme. I have drawn such a scheme for the particular case of eating. See Fig. 1.

In this diagram the intervening variables are shown inside the dash-line rectangle. The independent variables are shown outside at the top and left. And the final dependent variable, the behavior, is shown outside at the bottom.

Beginning at the upper right hand corner, the demand for consummation symbolized by Δ_c, is shown as resulting from, and varying primarily with, the values of M_f, a feeding maintenance-schedule. Under M_f I subsume such facts as: the usual time-interval between meals; lapse of time since the

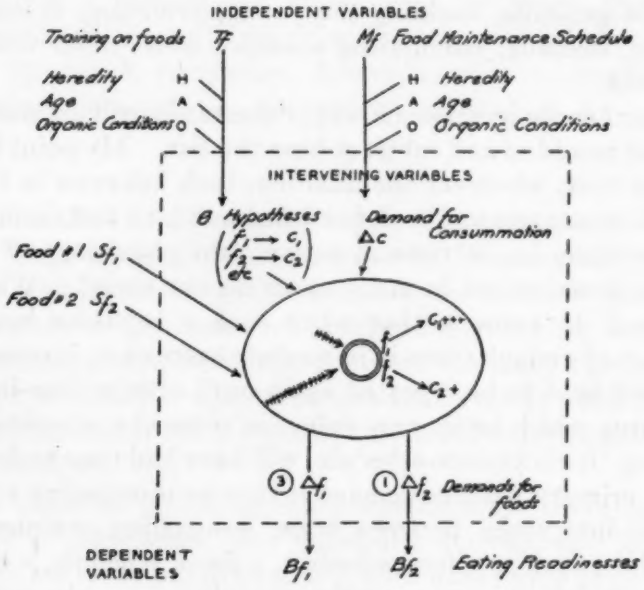

FIG. 1.

just preceding meal; the general dietary regime as regards constitution and amounts of food; and the like. Different values of M_f will give different resulting values of Δ_c. Finally, however, the exact quantitative effects of M_f upon Δ_c will be also affected by such individual factors as heredity (H), age (A), and special organic condition (O).

Turn, now, to the left side of the diagram. $\theta(f_1 \rightarrow c_1, f_2 \rightarrow c_2$, etc.) is a set of hypotheses to the effect that the foods, f_1, f_2, etc., will lead to consummations c_1, c_2, etc. And this

set of hypotheses, θ, I have represented as determined primarily by specific previous training T_f on such foods but also as affected, again, by the individual-difference factors of *H*, *A*, and *O*.

We note next that the θ and the Δ_c are to be conceived to combine together with the presentation of actual food-stimuli S_{f_1}, S_{f_2}, to produce a means-end field (depicted in the oval). This means-end field is to be imaged after the pattern of Lewin's 'psychobiological field' or 'life-space.' As here drawn, the arrows to the right of the pair of small concentric circles (which latter represent the organism) express a 'cognitive structure' in the field. These arrows indicate that the organism expects the consummations c_1 and c_2 from the two presented foods f_1 and f_2. And the angle between these arrows indicates the degree of differentiation which the organism achieves between f_1 and f_2 as a result of S_{f_1}, and S_{f_2}. The plus signs indicate the valences for the respective consummation c_1 and c_2 which result from Δ_c. Finally, the crossed arrows, in this case to the left of the organism, indicate the final resulting vectorial forces pushing the organism towards f_1 or towards f_2. We may call these final readinesses or demands Δf_1 and Δf_2 respectively. The former we have depicted as having, in the present case, a value of ③ and the latter a value of ①. The arrows issuing from the bottom of the oval merely depict again these same demands or readiness for food f_1 and f_2. And they determine directly the respective readiness of the behaviors—B_{f_1} (eating of food f_1) and B_{f_2} (eating of food f_2).

But a critic may here object that since the only dependent variable which one actually measures is always the same—namely the final resultant readiness-to-go-off of the behaviors B_{f_1}, B_{f_2}, etc.—it would seem impossible, 'operationally speaking,' to postulate more than one 'intervening variable' as determinative of, and defined by, this one final behavior result. It appears, however, that what we actually do is to choose separate 'standard' experiments for defining the several separate intervening variables.

For example, when we are defining the intervening demand Δ_e what we do is to conceive T_f and the S_f's held constant at certain standard values and then vary M_f only. Under these conditions we define the correlated variations in the resultant B_f's as 'mirroring' and measuring directly the variations in Δ_e. That is to say, we get a curve for variations of B_f with M_f and then we say that this curve really mirrors the variations of the intervening demand Δ_e with M_f. So that thereafter, whenever we have values of M_f, we can look them up on this curve and obtain the presumably corresponding values of Δ_e, even though under these later, non-standard conditions, this Δ_e is no longer to be conceived as appearing directly and linearly in any single behavior.

Turn now, to the set of hypotheses, θ. To define these we develop another standard experiment. In this experiment we hold M_f and the S_f's constant at certain standard values and vary T_f. And again we find correlated variations in B_f. But under these new conditions we say that these variations in B_f now define and 'mirror' directly the variations in θ, that is to say, in $f_1 - c_1, f_2 - c_2$, etc. In other words, we now have a curve or a family of curves between different values of T_f and different resulting sets of hypotheses, $f_1 - c_1$; $f_2 - c_2$; etc. Whenever, therafter we have different values of T_f we can look them up on these curves and postulate what this corresponding set of hypotheses, θ should be.

So much for a very brief and inadequate account of the definition of the primary positive demands and their accompanying hypotheses. Let me turn next, very briefly, to the *primary negative* demands. These are defined by final avoiding behaviors.

As a tentative list of such final avoiding behaviors I would suggest: avoidance of sudden loud stimulus-objects, avoidance of hot stimulus-objects, avoidance of cold stimulus-objects, avoidance of painful stimulus-objects, avoidance of movement-hampering stimulus-objects, and avoidance of fatiguing or exertion-requiring stimulus-objects. Each such type of final avoidance and any others which may eventually be added to the list expresses a negative demand against physiological

injury. It also involves an hypothesis that the given type of a stimulus-object is going to lead to such injury. Let me indicate this by another diagram. See Fig. 2.

This diagram is similar to the previous one except that the independent variables are represented here at the bottom and the dependent behaviors at the top. The final avoidance behaviors, B_{ap_1}, B_{ap_2}, etc., are conceived to result from a combination of the demand Δi against injury (shown at the right) and the set of hypotheses ($p_1 \rightarrow i_1$, $p_2 \rightarrow i_2$), that certain types of immediate stimulus object, S_{p_1}, S_{p_2}, etc., are going to lead to injury.

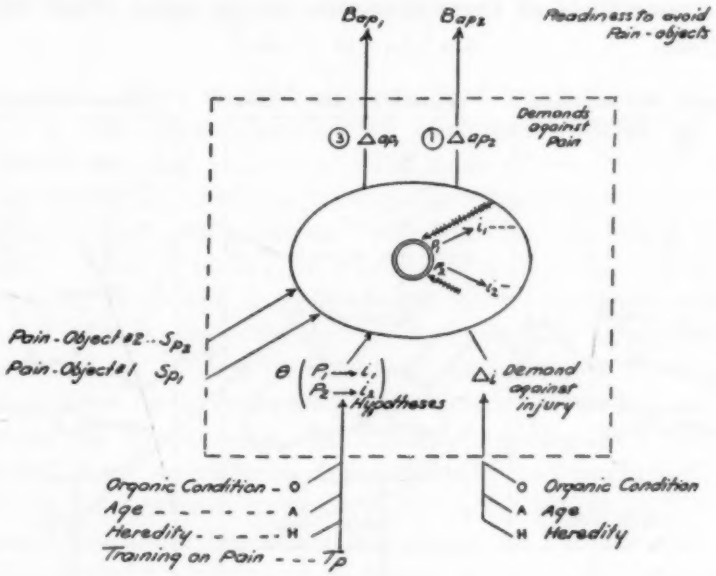

FIG. 2.

And defining experiments for establishing the relationships between H, A, O and the resultant Δi's and between T_p (together with H, A, and O) and resultant θ's could be set up in analogous fashion to those for the positive demands.

2. The Secondary Demands and the Mechanism of Their Attachment to the Primary Demands

Consider a concrete example, the experiment of K. A. Williams (7). She first taught her rats a white-black dis-

crimination box in which the white alley led to food. She then taught them a maze with this discrimination box as the goal at the end of the maze. And her experiment indicated that by virtue of the initial training on the box there was acquired and maintained for some time a demand for this discrimination box, as such. There also, presumably, were developed demands for the maze alleys. That is to say, if we conceive the primary demand as Δ_e, that for consummation, we may postulate a succession of secondary demands: Δ_f, demand for food; $\Delta_{b_1 b_2}$, demand for the discrimination box; and $\Delta_{m_1 m_2}$, demands for the maze alleys. And to indicate the interconnections of these demands let me again resort to a diagram.

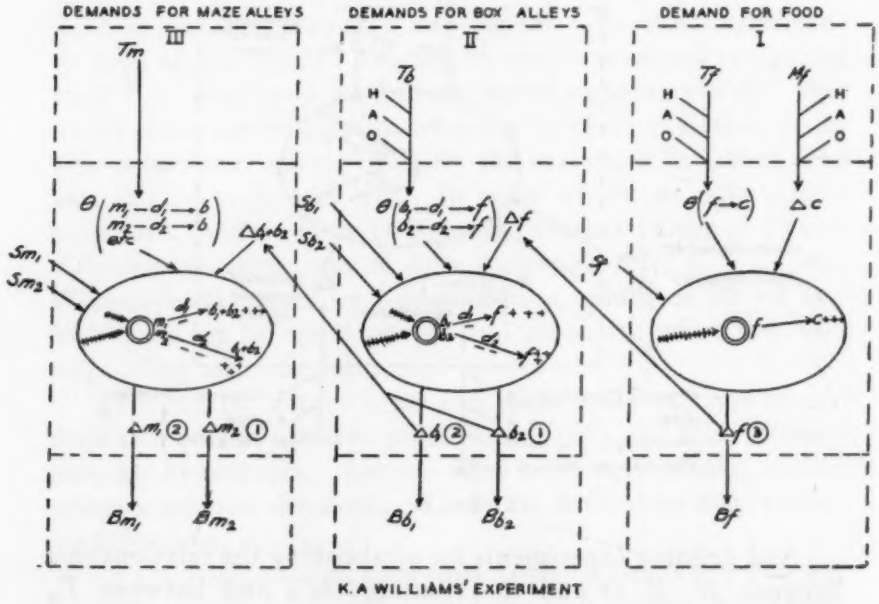

FIG. 3.

This diagram (Fig. 3) is built up of three successive units. Unit I is what was presented in the first diagram. It presents the relationships (established by standard experiments) whereby the demand for food Δ_f results from the demand for consummation Δ_e plus an hypothesis $\theta(f \rightarrow c)$ that the given

food leads to consummation. Unit II presents the further relationships whereby this demand for food Δ_f combined with an hypothesis set,

$$\theta\begin{pmatrix} b_1 - d_1 \rightarrow f \\ b_2 - d_2 \rightarrow f \end{pmatrix}$$

that the discrimination-box alleys b_1 and b_2 lead by distances, d_1, d_2 to food, results in the further demands $\Delta_{b_1 b_2}$ for these discrimination-box alleys. Finally, Unit III indicates the further relationships by which these demands for the discrimination box alleys b_1 and b_2 combine with the hypotheses,

$$\theta\begin{pmatrix} m_1 - d_1 \rightarrow b \\ m_2 - d_2 \rightarrow b \end{pmatrix} \text{ etc.,}$$

that maze alleys m_1 and m_2, etc., lead by distances d_1, d_2, etc., to the discrimination box and result finally in the demands Δ_{m_1}, Δ_{m_2}, etc., for specific maze alleys.

It should be noted, in passing, that this diagram is not so different from such a one as Professor Hull (**2**) might have drawn. That is to say, it consists of a succession of steps ending up with the final goal activity at the right. But instead of these steps being interconnected by hypothetical 'conditionings of anticipatory goal responses,' I have connected them by hypothetical 'hypotheses.' And the reason I prefer my hypotheses to his conditionings is that I think I can define my hypotheses experimentally and operationally, whereas he, I believe, can not do the same for his conditionings. I can state experimentally just how the demand for food varies with training on particular foods—this defines my first hypothesis. And I can discover and state experimentally just how the readiness to choose the correct side of a discrimination box varies with training on that box as leading to food—this defines my second hypothesis. And I can discover and state experimentally just how the choice of the correct paths in the maze varies with training on all the maze paths. This defines the third hypothesis. But *he*, so it seems to me, could not discover or state in equally operational and experimental terms whether or not there were anticipatory goal-responses and whether and how these latter were

conditioned to the earlier parts of the discrimination box and maze.

As to Professor Guthrie's (1) conditionings I have a suspicion that they are more nearly identical with my hypotheses, only stated in different words.

Further, I would point out that not only may we have chains of positive demands but we may also have chains of negative demands. A chain of two such successive negative demands is shown in the next diagram. See Fig. 4.

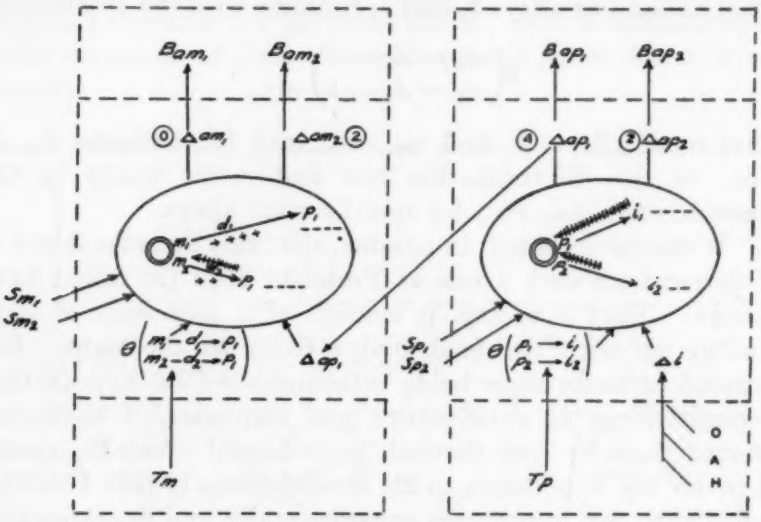

FIG. 4.

In this diagram the demand against injury, Δi, combines with the set of hypotheses $\theta(p_1 \rightarrow i_1,\ p_2 \rightarrow i_2)$ that pains p_1 and p_2 lead to injuries i_1 and i_2, and we get resultant degrees of avoidance Δap_1, Δap_2. And then the demand Δap_1 combines with training that a particular maze alley m_1 leads to pain to produce a further avoidance demand Δam_1 against such a particular maze alley.

Finally, our last, and perhaps most interesting problem concerning the secondary demands (and one that I can barely touch upon here) concerns the fact that these interconnecting hypotheses which connect a chain together seem sometimes to have (or to acquire), especially in human beings, certain

unique properties. I am thinking of such phenomena as 'fixations' and 'sign-magic' (5).

Consider fixations. These I would see as cases in which hypotheses normally 'docile' (5), namely, sensitive to the facts of further training, become rigid and fixed—or (to borrow from conditioned reflex terminology) become peculiarly resistant to 'experimental extinction.' When this has happened, the secondary demand (positive or negative) continues in spite of the fact that repeated new occasions are presented to the animal in which intercourse with this secondary object now does not lead to the primary one. But the animal nonetheless persists in seeking (or avoiding) intercourse with this secondary object. The interesting and significant question is, of course, what are the causal factors and the laws underlying such fixations. Both psychoanalysts and animal psychologists have remarks to make on this point, but I have not time to present them here.

Consider also 'sign-magic' (5). This I would define as the case in which intercourse is had, not with a real means but with merely an associated object—a symbol. The interconnecting hypothesis in such a case is a fake one. And yet (and this seems to be an important aspect of the situation) because the ultimate goal is forbidden—that is to say, because there is also working an avoidance-demand as regards this ultimate goal—the organism compromises by having intercourse, not with a true means-object that will really lead to this final goal, but with a mere symbol of this goal—something which looks like, or is part of, or merely fortuitously and conventionally associated with this goal. In most cases, of course, this sign-magic mechanism has to do with erotic needs. The patient is for some reason forbidden the actual sexual goal and instead of seeking an actual mate has commerce with other objects (*e.g.*, drawings or fantasies) which function as signs for the sexual goal. The patient does to the sign what he would like to do (but is afraid of doing or is in some other way inhibited from doing) to the ultimate goal.

But this suggests finally our third problem, that of interaction among demands. The phenomenon of sign-magic is, in short, one of the phenomena which may result from conflict.

3. Competition Between Demands. Conflicts

Lewin seems to me to have developed the best set of concepts which we yet have for describing conflicts. I have indeed already indicated this in the preceding diagrams. No demand, even in the simplest case, functions in isolation. It operates as but one vector among others in some total, larger, psychobiological field. When, however, as then, we were seeking standard experiments for separating out and defining

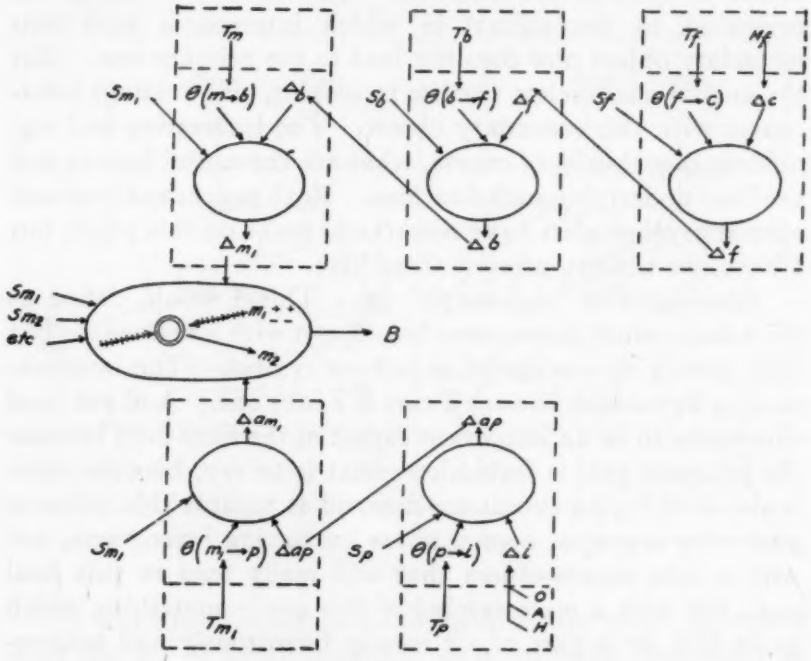

Fig. 5.

individual demands as such, I made the field-situations as simple as possible and largely ignored any special field dynamics which may have been involved in determining the final behavioral expression of these demands. When, as now, however, our interest is in these very field problems themselves, it appears that a whole new set of concepts must be evolved.

Figure 5 presents the case for a rat which is conceived to have both a positive demand for maze alley No. 1 as a result

of training such as that in Williams' experiment and a negative demand for this same alley as a result of some other special training with electric shocks in such a type of alley. The definitions and historical determinations of these two demands are indicated by the upper and lower series of rectangles, respectively. And the final result is shown in the middle and at the left. This is the final means-end or psychobiological field which results from these two sets of training and from the actually presented maze alleys.

And it appears that in order to predict the final behavior from such a field one must develop, as Lewin (3) has done, a whole set of new concepts such as: 'encapsuling,' 'going out of the field,' 'going into an irreality plane,' 'sign-magic,' 'tensions,' 'breaking down of the walls within the personality,' etc. I have, however, no time here for a real discussion of such concepts. I wish merely to assert that these would be the general types of concept which I would have recourse to in discussing conflicts.

To conclude, I wish to emphasize again that all that has been attempted in the above discussion is: (1) to indicate the actual types of variables with which we 'operate' in our different experimental attacks on motivation and on conflict; and (2) to suggest that geometrical schema are possible for helping us to keep before us the resultant interrelationships and the inevitably postulated 'intervening variables' clear and disentangled.

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THE FUNDAMENTAL PRINCIPLES OF TYPOLOGY

BY HERBERT SHUEY

State Industrial School for Boys, Topeka, Kansas

The study of personality and human relations has become increasingly important during the last few years. Along with this increased interest comes the necessity of grouping and classifying individuals for more accurate study and comparison. The changes in scientific methodology have brought different approaches to the problem of classification, and on the basis of these changes we have the development of modern typology (*cf.* Shuey, **11**). The science of human nature needs a unit and a classification. There must be some fundamental basis for grouping individuals, otherwise chaos will reign instead of order.

Typologists are almost universally identified with the *practical* application of psychology to the study of human behavior. Hippocrates, Galen, Jung, Kretschmer, Rorschach, W. Jaensch, Draper, Pende, Naccarati, De Giovanni, and many others are medical men and psychiatrists. It is highly significant that these practical men have developed typologies, whereas psychologists, who have always been accused of being impractical, have never developed typologies of any importance (see Murray, **8**). But typologists have been slow in giving philosophical or even biological justification for their types, and the academic phases of psychology so necessary for a typology are often quite foreign to them. Their long contact with behavior problems has led them to establish typologies, that is all. They have usually selected a typology in accordance with their particular field of interest, not a universal typology to be applied in all fields. This has led to a limited application of their data, since it requires the acceptance and understanding of their particular brand of typology. These men have often been quite critical of

academic psychology, and they have objected to its impractical nature. The psychologist in turn has retorted that these practical men have been unscientific and subjective.

While the reaction against older academic psychology has produced new movements in psychology, these movements have not been wholly typological. Psychologists who have followed the constitutional theorists and psychiatrists in studying behavior have invariably taken over a typology. The Gestalt psychologists and field theorists have not done so.

The reason for this important deviation in the newer movements in psychology lies in a difference of aims. The Gestalters and field theorists are chiefly concerned with methodological questions. In so doing they become more general and are considering a large field of which the individual is only a part. The individual for the field theorists becomes a point in the field. The Gestalt psychologist points out that the individual is a configuration, but he gives no experimental studies that would lead to the practical application of this principle to the problems of the individual, especially specific problems of everyday life. He is concerned with perception, learning, and with much the same things that made up older psychology. About the only change that has taken place, from the practical standpoint, is that the emphasis is placed on the whole as opposed to the parts, and configurational studies have replaced the older analytical and atomistic experiments. For this reason the type concept has not received much attention in the Gestalt school.

This has also tended to lead the Gestalters and field theorists to overemphasize the environmental forces. Any system that does this tends to stress universal law and to relegate the parts to a subordinate position. The field is the object of study, not the individual making up a part of that field. Therefore types are not used.

The typologist is not denying the interrelationship of things, but he points out that there is a biological structure that maintains the individuality of each person throughout life. This structural basis is similar to that of other individuals, hence the type concept; and a study for practical and

even theoretical purposes must be more concerned with the individual than with the infinite ramifications of universal law.

Köhler (**6**), one of the founders of the Gestalt school, has recognized this situation and takes a much more liberal view than some of the more radical Gestalters. He states on page 153:

> One view of the physical world would be: nature consists of *and-connections*, of independent parts whose purely additive totality makes up reality. A second and opposite view would be: in nature there are no independent parts, but all conditions and events are only real in the total world connection, all 'parts' are abstraction products. The first statement is entirely incorrect; but also the second hinders rather than promotes an understanding of the *Gestalt* principle.

Köhler goes on to illustrate this point with radium. The disintegration of radium has been little influenced by anything man has been able to do. He points out that while there may be some remote relationship with the universality of nature, this relationship shows degrees of strength.

The typologist would subscribe to the latter stand. While the individual and the environment are not separated, certain aspects of the individual structure show far less dependence on environmental changes and maintain themselves in a more definite form throughout life than other aspects, especially more so than the so-called faculties and character traits. Such structures as the fingerprint patterns, the curvature of the scapulae, and other biological structures can almost be considered permanent, although there is constant change both in the environment and in the individual. These biological patterns that show a similarity in groups of individuals form the basis of the type. The behavior patterns, such as described by Jung and other typologists, are then correlated with these biological patterns. In many respects this gives a far more complete picture of the individual than the field theorists or Gestalt psychologists have been able to give.

Some very interesting work has been done by the field theorists. Typical of this is Dembo's (**2**) study of anger. The field is described and the variations in behavior of individuals faced with an impossible problem are given. Such terms as 'breaking out of the situation,' 'shifting the degree of reality,' and other terms are used to explain the changes noticed. But the typologist is not satisfied with this post-descriptive type of experiment. Why do some individuals break out of the situation and show anger, why do some shift the degree of reality? The typologist seeks to find this in the fundamental biological structure of the various individuals. If he finds that some persons have a similarity of biological structure and also show a similarity of behavior patterns, he can predict what the individual would be likely to do, whereas the field theorist can only describe what has taken place *after* it has occurred.

This explains why many typologies that have come from academic psychology have not been practical. They lack a biological basis, hence cannot be correlated with the mass of pathological and clinical material that is available. Sociological types also lack this biological basis and sociologists are faced with the same difficulties.

It is natural that with the development of typology many criticisms would be offered, some of them repeatedly (*cf.* Klüver, **5**). Let us give these criticisms since they must be faced if typology is to become scientific.

One criticism concerns the distribution of individuals according to types. It has been argued against typology that individuals when tested for some variable or variables fall on a normal curve; therefore there are no types. Or it is objected that typologists are setting up bimodal or trimodal curves of distribution which do not exist in nature; therefore typology is based on an unnatural classification. Further, it is argued that most people do not represent clear types, but mixtures of types; therefore types are superfluous and impractical. A corollary of this is that when a classification is once established, an increasing number of types are found until almost every individual represents a type and the classi-

fication becomes impractical. Another argument is that the situation determines the response, so that the individual in one situation belongs to one type and in another situation to another type, which makes a typology impossible. Again, it is argued that the organism is in constant change; therefore one type may change into another, which makes a typology impossible over a period of time. A variant of this is the question of how a type can represent a fixed pattern when it is a part of a dynamic system; or, putting it in more philosophical language, how it is possible to have permanence in a system of change. Then there is the kind of criticism that rejects typology without giving any reasons. Types are declared to be unscientific, or are said to be non-existent. This is more an attitude than a criticism, but is common among many psychologists and even psychiatrists.

Before these criticisms can be answered, we must consider what the fundamental principles of typology are.

1. The Type As an Expression of the Total Organism

Older psychology has almost always worked with parts of the individual. Traits, separate performances, faculties and so on have been studied and grouped. Thus, sensory and motor forms of responding have been found, studies of intelligence, ambition, speed of tapping, perception, touch, etc. have been made, and types have been established on the basis of these data. But these psychological types cannot be applied to behavior problems. The reason for this is that a part of the individual has been isolated. While it is possible to establish types, or better, to group individuals according to any one trait, it has always resulted in failure when the attempt was made to correlate these various groups or types with one another, or when the attempt was made to form a general type from the single types. The totality of the individual, the individual as a whole, has not yet been typed. The only justification for this kind of work was often the statement that it was empirical and 'objective.' We can see that modern typology must stand on the principle that the type should include or represent the total structure of the

organism and cannot be built up entirely empirically. *The individual can therefore belong to only one type in respect to the totality of his structure. He may belong to many types when parts of this structure are considered.*

This fact is demonstrated in the use of the introversion-extraversion concept. A common procedure is to take the traits which are supposed to be characteristic of these types and make them into a questionnaire. The assumption is that an introvert will mark the introvert traits and the extravert will mark the extravert traits. The original idea of Jung (4) concerning the direction of the libido with its compensatory mechanisms seems to be entirely lost in this attempt. The results from such studies have been very unsatisfactory.

This is shown in a study by Page, Landis, and Katz (9). They made a study of questionnaires made up of traits considered typical of schizophrenia and manic-depressive psychosis. These traits were considered typical by 100 per cent of the psychiatrists who acted as judges. In general it was found that when these questionnaires were given to schizophrenics only about 50 per cent of them showed a schizophrenic shut-in type of personality. Often normal individuals showed more schizophrenic traits than schizophrenics themselves.

2. Dynamic Features of Traits

An important principle which has not been considered at all in most of the work on personality arises from the dynamic nature of many traits studied. Body build, handwriting, shape of face, and other bodily features have been correlated with ambition, intelligence, honesty, pleasing personality, and other personality traits. No recognition of an underlying dynamic difference has been made. Ambition, honesty, and such traits are partly constitutional and partly environmental. Changes in the outer environment can cause radical changes in the traits. The boy who shows no ambition through half his life is suddenly spurred into an unbelievable ambition towards some goal by an accidental event occurring in his life. The changing dynamic quality of these traits makes them

entirely unsuited to correlate with more permanent constitutional factors.

Another difficulty in dealing with such traits is the lack of objective criteria to determine the presence of the traits, since part of the trait is dependent on the momentary condition of the environment. A common method is to use the judgment of three or four laymen who know the individual to be tested. After this unscientific step has been made—unscientific because the influence of the environment has not been included in the judgment—the statistical studies assume a rigidity and objectivity worthy of higher mathematics; but they only conceal the original unscientific judgment of the trait.

3. Phenomenal and Structural Analysis

A constantly overlooked principle in modern personality work has been the confusion between phenomenal and structural analysis. Two people may show the same phenomenal behavior, yet when both individuals are studied, it is found that the reasons and causes back of the behavior were entirely different. The Freudians (3) very early recognized this principle and much of their success has been the result of a more perfect structural analysis. In fact, one might say that this analysis is one of their chief scientific contributions to the field of personality.

Many psychologists who have wanted to preserve the so-called 'objective' school of psychology have attempted to study only the appearances, and have shut their eyes to subtle causes of the behavior. They measured the behavior as they found it and failed to realize the underlying structural differences between similar forms of behavior. Only psychiatrists, psychoanalysts, and clinical psychologists are on the look-out for such differences. In typology this principle is emphasized more than ever, because the type differences are structural differences and similarity in behavior may be on the basis of entirely different structural and type factors. Dembo (2) and other field theorists make no mention of this situation. In typology overt behavior as such is not considered in determining the constitutional type.

4. Compensatory Mechanisms

This principle is violated especially by those psychologists who use the questionnaire and layman judgment methods in their studies. Close students of human nature know how important compensatory mechanisms are. The individual compensates for defects, inadequacies, and even for the normal functioning of the personality. The extraverted individual who is overly sociable imagines that he is an introvert and will often mark more introvert traits on a questionnaire than he should, simply because he is unconsciously compensating for his highly sociable nature. The introvert makes similar compensation. Jung, of course, pointed this out in his original work. Why the personality workers have forgotten this and use such traits in questionnaires, assuming that the introvert will always cross out or indicate introvert traits because he is an introvert, is hard to understand.

The same fact of compensation is true of the judgment of another person's nature. It is assumed by the personality workers that if three or four individuals judge another individual, their judgments will offset any compensatory mechanisms. Whether this actually takes place or not they never bother to investigate. They merely take the judgments and then hurry to the complicated correlational calculations which make up the bulk of the work. In typology this type of research is violently condemned. The important part is not whether the correlational studies are correct or what they prove, but whether the judgment of three or four individuals of another individual is correct. The study should be concentrated on this problem before the other is undertaken.

The only way to avoid compensatory mechanisms in personality work is to avoid questionnaires that assume a correlational value between what the individual marks and what he is. Before such an assumption could be made, a check would have to be made by some other method to determine whether a pure introvert, for instance, would always mark introvert traits. This was assumed with the schizophrenics, but the correlational studies have not sup-

ported the assumption; if anything, they have disproved it (see Page, 9). The same can be said of judgments. The individual judging has his own mechanisms and judges an individual in the light of these.

The typologist avoids compensatory mechanisms because he is interested in constitutional reactions that are to a high degree unknown to the average individual. He is more interested in fundamental actions than in questions that involve complicated psychological and compensatory mechanisms, at least in the determination of the type.

5. The Polarity Principle

We come now to the last important principle for a scientific typology, the principle of polarity. Since this principle has not yet been discussed fully in psychological literature, the following description is given in some detail.

We are constantly faced with characteristics that show opposite tendencies. One and the same individual can show love and hate, pleasure and pain, aggression and submission, introversion and extraversion, sadism and masochism. Individuals fluctuate back and forth between two poles, and the normal individual can adjust his behavior so as to display the right type of reaction at the proper time. When it is necessary for him to be aggressive he is aggressive, and if the next minute he must let his superior step in front of him, he shifts from his aggressive state to the submissive state. In the small child these rapid shifts can be observed in all simplicity. Emotional changes have long been known to follow such polar changes (see Marston, 7). An individual who hates another finds this hate swinging in the opposite direction and he is loving the other just as passionately. The same is true when his love turns to hate. There is so much in life that fluctuates this way that any static measure of the individual at a particular time becomes difficult. Concepts like high and low, hot and cold, broad and narrow, good and bad, ramify through our language and are used successfully in spite of the transitions between the extremes or poles and in spite of the

difficulty of determining the extremes 'objectively.' For this reason tests must take this polarity swing into consideration.

Biologists have also used the polarity principle in their study of sex (see Weininger, **13**; Thesing, **12**). Male and female represent poles and it is sometimes possible by transplanting glands to change the sex. The effects of castration are well known. If the sexes were fundamentally different, such changes would be unlikely. We have here a biological polarity of sex, but after it is once established, it is maintained in a fairly constant state. Old age (due partly to glandular changes) causes a swing in polarity, and the old woman often takes on masculine characteristics (deeper voice, hair on face) and the old man becomes somewhat feminine (high quavering voice, emotional outbursts, garrulity, emotionality, deposition of fat resembling that of a woman). While the sex organs may belong to one or the other sex, the individuals often show a good deal of mixture in their masculine and feminine components. Thus a man with good primary and secondary sex characters will often show a predilection for perfumes, feminine expressions, and even a liking for his own sex (homosexual). We have not only two poles but mixtures between the poles. Thus, individuals fall on a normal curve of sexuality, although they are more or less sharply differentiated in the primary sex characters.

We see here an explanation of why the attempts to find differences between the sexes have resulted so often in failure. According to the polarity principle the sex characters are distributed on a normal curve and the majority of individuals will show little difference. It is only when the extremes are viewed that such differences become important. The endless arguments about whether men or women are superior rest on such a misconception of the true state of affairs. Tests given to groups show no statistical difference in performance between the two sexes. It is therefore concluded that there are no differences. The predominance of men in creative work (music, mathematics, philosophy, and the higher forms

of literature) would be confined to these extremes at the poles. If only the extremes were tested in the things that actually differentiate the sexes, differences would most certainly be found. Since creativeness is considered a distinctly masculine trait, some psychologists have attempted to measure creativeness in both sexes. The results were obvious. No differences were found in the statistical distribution, since the majority of people fall in the mixed groups and the extremes balance one another.

Emotionality is considered a distinctly feminine trait, yet when emotionality is tested for in groups, no significant—or only slightly significant—differences are found. Here again the majority of individuals show a mixture of their emotionality and some men will be more emotional than some women, and *vice versa*.

The conclusions from personality investigations of this kind have bordered on absurdity, not because they did not find differences, but because they concluded that no differences existed because they could not find them. The polarity principle and the statistical distribution of traits were never discussed in these experiments. And yet they were considered 'objective.'

The numerous studies of introversion-extraversion illustrate this neglect quite clearly. Introversion and extraversion are polar opposites and follow the polarity principle. Tests devised to determine these two mechanisms have usually been of the static kind and have not taken the swing into consideration.

The polarity principle has not been unknown in psychology. Freud (3), Apfelbach (1), Seifert (10), Jung (4), and many others have mentioned this mechanism, yet it has not received the attention it should have in general psychological work. At the present time, however, with the introduction of the time concept it has again appeared with renewed importance (Seifert, 10, p. 42).

Conclusion

In conclusion let us turn back to the criticisms offered. The answers to many of these follow of necessity in the light of the principles just given.

The first criticism is concerned with the distribution. Types will fall on a normal curve of distribution because most biological measurements do fall on such a curve. The type is not a rigid pattern, but can be considered as a position on such a normal curve. The extremes represent two types, and the middle portion represents the mixed group or mixed type.

Bimodal and trimodal curves are not obtained in biological constitutional studies. Typology follows biology in this respect.

The fact that most people belong to mixed types is not only accepted but is considered necessary for the reason that we are dealing with a normal curve of distribution with half of the individuals at least falling in the middle groups. Pure types are rare.

The criticism that every individual represents a different type rests on the fallacy of phenomenal and structural analysis. Phenomenally every individual differs. Structurally there are differences, but along with these are very marked similarities of biological patterns. These similarities are grouped as types in typology. The criticism assumes that there are no similarities of biological structure.

The criticism that the situation determines the type and that an individual in one situation is one type and in another situation is another type also violates the principle of phenomenal and structural analysis. Phenomenally he differs in different situations. Biologically he does not change. It is just as absurd to say that a man is a woman in one situation and a man in another. Even when a man puts on women's clothes and walks and talks like a woman, he is still biologically a man.

The criticism as to change and permanence of types harks back to the days of Heraclitus and Parmenides. Today older psychology represents the static position of Parmenides

and change plays little part in their theory and discussions. On the other hand, the radical modern psychologists are fast approaching the extreme position of Heraclitus' followers and they are becoming so dynamic that everything is change and nothing is permanent. Typology takes the stand that there are degrees of change, and types must be based on those structures showing a low degree of change although they are never considered unchanging.

The criticism that there is disagreement between typologies and that there are so many different ones neglects the important fact that the startling similarity between the constitutional typologies indicates that they are all discussing the same things. The description of the integrated type of Jaensch, the hypoevolute type of Pende, the pyknic type of Kretschmer, the extratensive type of Rorschach, the extraverted type of Jung are almost indistinguishable.

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A NOTE ON PROFESSOR TRYON'S THEORY OF INTERCORRELATION

BY BRONSON PRICE

Ohio State University

In place of 'mathematical factors,' Tryon has presented a theory (**6**) [1] which views positive correlation between two tests as conditioned by (*a*) extent of community of components, (*b*) degree of correlation between the environmental fields in which the components sampled by the two tests originate, and (*c*) degree of correlation between gene-blocks determining the somatic tissues brought into action by the two tests. Concerning (*c*) Tryon writes, " . . . men and women of genetic superiority for achievement in quite *different* domains tend to interbreed, as well as do those of diverse inferiority. On the genetic side, what happens, then, is that individuals possessing blocks of genes determining superior capacity for one type of achievement unite with individuals possessing blocks for superior capacity in another. Their offspring, in consequence, tend to receive genes for superior capacity in *both* types of achievement. . . . Whereas the genes affecting different abilities are physically independent, this type of assortative mating produces a positive correlation in their phenotypic expressions." Due to difficulties of foreign study, the present writer failed to see this until after he had published a later paper (**3**) on much the same point. The effect was expressed in terms of cross homogamy, in distinction from homogamy tending only to increase variability, and in consequence of which intercorrelations were seen to reflect the attributes valued in the particular society or group studied.

Concerning (*b*) Tryon refers to 'systematic differences

[1] It has also been brought to the writer's attention that Tryon described the points concerned in his chapter on individual differences in 'Comparative Psychology' (F. A. Moss, Ed.), 1934.

among individuals in the general quality of the educational and cultural medium in which the different concepts are evolved.' The effects of non-genetic differences between social classes in raising intercorrelations were referred to in (3) as aside from the point; the writer would now agree that (*b*) deserves no less attention than (*c*).

In relation to (*a*), the points that 'no simplified set-up such as that postulated by the *G* and *P* (general and group factor) theories occurs genetically,' that 'a mathematical factor cannot be conceived of as *an* hereditary factor,' and that non-cumulative interaction of genes prejudices the case of the factorist, are believed to warrant further discussion. These may be true in part, but such aspects of genetical interaction as are called pleiotropic effects may also be said to provide theoretical grounds for the factorist's efforts. However much the factorist has oversimplified his case in relation to genetical structure (and it will be agreed that here statistical description is invaluable), it should be granted that there probably exist genetical sources of intercorrelation in some degree similar to so-called general and group factors.

Psychological 'components' and 'factors' both refer to variates, and we are provided with exact frames of reference for variates in terms of underlying pairs of allelomorphic genes or 'alleles.' In theoretical discussions we can speak with no less confidence, and perhaps with less confusion, in such terms than in terms of components or factors. The writer supposed in (3) that the 'overlapping' of alleles and the effects of genes acting like general factors were different, and, if he has correctly understood Tryon, (*a*) has been presented with an analogous view in mind. However, Professor H. J. Muller has pointed out to the writer that distinctions between alleles assumed to be common to two or more functions and alleles assumed to affect several functions are ambiguous or arbitrary. In the light of the considerations above, it is believed that the separable causes of observed intercorrelation may be termed, (*a*) communality or pleiotropy of alleles, (*b*) environmental class differentials, and (*c*) intercorrelation of alleles in the population owing to cross homogamy.

Thurstone has expressed the conviction that the isolation of the mental abilities will turn out to be essentially a problem in genetics. He has provided a schema (**5**) which views 'unitary factors' as uncorrelated, and which would account for intercorrelation in terms of communality. Kelley has felt that a genetical 'very many factor view' is logically defensible only if practical issues of utility are neglected (**1**). Thompson's views have been compatible with biological considerations, though it is questionable whether correlations have 'been produced by that process of sampling which we call the survival of the fittest' (**4**). Segregation would of course markedly inhibit the accumulation of correlation gained solely in this way, and it is not clear that differential survival should especially favor just those types whose selection would effect correlation. Spearman, it appears, would have had genetics overlooked and the sufficiency as well as the necessity of his constructs accepted; if this had happened theory would have been to that extent 'vitiated,' in the term of his own hyperbole.

The theoretical and experimental considerations set forth by Tryon are believed by the present writer to be very much to the point. Tryon has felt that crucial evidence concerning agencies (*a*), (*b*), and (*c*) can be secured only in the animal psychological laboratory. However, effects (*b*) in man are essentially what modern twin techniques distinguish as inter-familial environmental influences. Twin,[2] family resemblance, and foster-child methodology should be capable of extension to studies permitting the estimation of covariance ascribable to (*b*) as apart from that ascribable to (*c*), and of providing estimates of intra-familial environmental effects if needed. The latter may become important in the realm of personality traits, but probably do not seriously complicate capacity functions as intra-familial variance in them is known to be small. For separating out effects (*a*), an approach like that in the following example is available.

[2] It should be noted that certain of the assumptions involved in contemporary twin studies, namely, that assortative mating and correlation of genotype with environment are negligible, and that the genotypical fraternal correlation is ½, are untenable and to be avoided in studying capacity traits.

The data of a resemblance study in Moscow showed the average of the marital coefficients for a vocabulary test and a number-series-completion test to be .66, while the average of the cross marital coefficients (husband's vocabulary score correlated with wife's number score, and vice versa) was .61. When this finding was thought to reflect cross homogamy as a source of intercorrelation, Muller called attention to the fact that data of such type could also be taken to represent a corollary of the general factor view. He suggested that the correlation of the functions within families of offspring be found, in order to discover the effects of 'general factors' alone. We cannot study a large number of offspring from a single mating, but we may combine observations on offspring of many matings. Considering only families having four or more children tested, 45 sets of offspring were available in the writer's data. Thus 45 correlations between the age-specific vocabulary scores and number scores might be calculated and averaged. Covariance analysis, however, yields the relevant information somewhat more efficiently, showing here an inter-familial correlation of .76, an intra-familial correlation of .42, and a total correlation of .63. The respective standard errors are .06, .07, and .04, approximately. The inter-familial coefficient is attributable to effects (*b*) and (*c*) combined, and is thought to be no less indicative of (*c*) than of (*b*) as class differentials [3] in this group of 204 children were not great. It thus appears that Tryon's argument concerning these effects cannot well be disregarded. The intra-familial coefficient represents an estimate of effects of the (*a*) type for these functions. In case chromosomal linkage raises such a correlation materially, this effect should be discoverable by methods proposed by Penrose (**2**).

If factor analyses were to proceed as practiced at present, the writer would hold with Tryon that the future of these studies is black. Investigations giving estimates of the

[3] Since misunderstanding exists, it is well to state that it is contended in the U.S.S.R. that temporarily necessary social categories have replaced social classes in the sense of owners and non-owners of the means of production. It has not been contended that all socio-economic differences have been 'leveled.'

magnitudes of each of effects (*a*), (*b*), and (*c*) are feasible however, and it is urged that a part of the current investment in factor analysis work be directed toward such undertakings. Applied, then, to the intra-familial correlations proposed by Muller, it is believed that factor analysis methodology may decide whether or not any few reference variates are clearly distinguishable in capacity traits, and thus be of considerable profit to genetical and psychological theory.

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